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SECTION OF GEOLOGY AND MINERALOGY

OCTOBER 7, 1946

DOCTOR NORMAN D. NEWELL, The American Museum of Natural History, and Columbia University, New York, N. Y.: *Geologic History of the Lake Titicaca Region, Peru.* (This lecture was illustrated by lantern slides.)

Lake Titicaca and the surrounding region in Peru and Bolivia form, from many points of view, one of the most fascinating areas on this globe. Among archaeologists, the locality is celebrated as the birthplace of some of the most remarkable ancient civilizations of the new world. To geographers, it occupies a place of exceptional interest as a great laboratory for the study of the struggle for existence under difficult conditions of environment. Geologically, also, this part of the Andes is of exceptional interest.

Lake Titicaca lies near the mid-point of the Andean Cordilleras, so that its geology has a strategic bearing on the geological problems of regions to the south and northwest. The stratigraphic sequence is enormous. I have measured a sequence, in post-Silurian rocks, of more than 12 miles of strata, around Lake Titicaca. Additional thousands of feet of Early Paleozoic strata are known to occur in adjoining areas.

This region lies near the apex of the great structural re-entrant of the Central Andes, responsible for the notch in the continental margin,

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the Gulf of Peru. This anomaly in the coastline has long intrigued students of South American geology. Examination of geologic evidence suggests that the margin of the continent has been mashed against a resistant massif now covered by the adjacent Pacific Ocean. Old injection gneisses crop out along the coast of the Peruvian Gulf at various places between Paracas, latitude 14° south, to Cabo Tablas, at 32°, near Valparaiso. The structural alignment in this basement complex is not parallel with the regional strike of the Andes. The grain of the later Andean folds is, however, parallel with the coastline. Existing data demonstrate a pre-Mesozoic, and probably a pre-Cambrian, age for these old rocks. Early Mesozoic marine strata overlie the basement complex at several places along the coast.

There are reasons for suspecting that the coastline of southern Peru lies very near a stable axis of tilting. The Krummel Deep parallels the coast and lies about 100 miles or less from the crest of the Western Cordillera. The present shore is about midway between the Oceanic Deep and the high peaks, and the shore has occupied about the same relative position since at least the Miocene. One is tempted to judge that the rise of the Andean Arch in this area has been very nicely compensated by the sinking of the Krummel Deep. If the continent were to be flooded by a eustatic rise of the sea level of 700 feet or more, the Andes would resemble some of the great island arcs of the Pacific region. The area is noted for active volcanism and deep and shallow earthquakes.

An area around Titicaca was systematically studied by me during approximately one year spent in the field. In many instances, the interpretation of the local geology so depended on knowledge of surrounding areas that many trips were made into adjoining areas, especially the coastal region and the area to the northwest of Lake Titicaca.

The Andes are very high, and very young, which has led to a popular misconception that they are also everywhere quite rugged. As a matter of fact, canyon cutting has by no means kept pace with uplift, so that there are vast areas of relatively level plateau upland in the Central Andes. This plateau is rimmed to the west by a chain of volcanic cones and bedded volcanics, forming one of the truly great volcanic areas of the world. The volcanic range, or Western Cordillera, includes many active or recently active volcanoes, some of which rise to elevations of around 20,000 feet above sea level. To the east, the Central Andes are bounded by mighty snow-capped ranges composed of early Paleozoic rocks. In some instances, these mountains owe their great relief to block faulting. In other cases, they are erosion residuals.

Erosion of the Pacific and Amazon slopes of the Andes is very unequal. The dry climate of the Pacific slope results in greatly retarded erosion, so that in some places the front of the Andes presents a gently arching surface with broad, flat divide areas. On the east slope, enormous canyons are being cut back into the mountains from the Amazon drainage. Some of these are profound chasms a mile and a half deep. On both sides of the Andes, canyons are encroaching on an upland sur- of late maturity.

This upland plateau, or Puna peneplain, is one of the most remarkable in the world. It was carved mainly in the latest Tertiary, following the last main compression of the Andes (Miocene?), and has been modified more recently by block faulting and by regional epeirogenic uplift to a present high level of around 14,000 feet. Most of the volcanics of the Western Cordillera are essentially horizontal on this old peneplain.

In southern Peru and Bolivia, block faulting has caused the local collapse and settling of the Puna plateau to form a great basin, one of the world's great systems of internal drainage, the Altiplano, in the northern part of which lies Lake Titicaca. During the Pliocene and perhaps the early Pleistocene, the Altiplano was occupied by enormous lakes, of which Titicaca, Poopó, and the great salars of Bolivia are the relicts. The deposits of these lakes and their tributary streams form an enormous lacustrine-alluvial plain, most of which lies at elevations of 11,000 to 13,000 feet above sea level, a thousand feet or so below the surrounding plateau.

Lake Titicaca is a fresh-water lake about the size of Lake Ontario, with a maximum depth of around 1,000 feet. It is deepest near the northwest border, where there is geologic as well as topographic evidence of block faulting. The shore is generally precipitous, with numerous drowned estuaries cut by streams, evidently when the lake had a lower level than at present. The southwestern margin of the lake, on the other hand, is characterized by broad alluvial plains, and a rather large delta (Rio Ylave) is built far out into the lake.

Due to the high altitude and other factors, the area around the lake has but a thin cover of vegetation. Consequently, the erosional processes and resultant forms resemble those of semi-arid regions.

Actually, the annual precipitation is said to be not far from 40 inches. The growing season, however, is short, and the altitude is too great for the majority of crops and for arboreal vegetation. During much of the year, the climate is unpleasant.

Evidences abound for a formerly great and flourishing, indigenous human culture, where now the environment is inhospitable. It is interesting to speculate on the possible physical changes that may have been directly responsible for the cultural decline in the region, during the last thirteen centuries or so.

According to the legends of the Incas, the first Inca and his wife came down from the sun to an island in Lake Titicaca. They made their way northwestward to a new region outside of the lake basin, near Cuzco, where they founded the first Inca dynasty. Some centuries later, about 1200 A D., during the rapid expansion of the Inca Empire, the warrior Inca, Mayta Capac, came as a conqueror to the southern end of Lake Titicaca. There he found impressive remains of an ancient civilization, which appears to have centered around the ruined city of Tiahuanaco. Living in the area was a primitive people who could give the Inca but little information about the ruins. The numerous vestiges of an advanced culture, particularly the impressive architecture, are said to have influenced the later Inca civilization in many ways.

This much is certain, that neither during nor since its domination by the Incas, did the area ever recover the former high cultural level enjoyed by the Tiahuanaco people. Evidences are abundant around the lake for a former very dense agrarian population. Impressive are the ubiquitous agricultural terraces on almost all hill slopes around the lake. These extend from lake level to elevations of around 13,800 feet.

The present population of the region is nearly pure Indian, direct descendants of the people who originally inhabited the area. Though degenerated through centuries of misrule and abuse, they are well adapted to the harsh environment in which they live. They still retain many of their ancient cultural traits, after four centuries of contact with Europeans.

Visitors in the region suffer, in varying degrees, from discomfort due to the high altitude. For many, the ascent from the coast to the Altiplano is very hazardous and, if not fatal, may at least be permanently damaging. For the majority of healthy young people, adjustment is rapid and complete. Normally three or four weeks of limited activity are needed before heavy exertion can be tolerated. The majority of white men who work in the high Andes descend to the coast, to rest, at six-month intervals.

Special conditions in the prosecution of my work permitted the adoption of field methods which enabled me to travel continuously, moving my camp each day. My pack train was sent ahead with instructions to make camp at a given point. These arbitrary camp sites were plotted on excellent topographic sheets of the Carta Nacionál of Peru. Unfortunately, many local terms which appear on the published maps are not in general use. My assistant and I had to search for our camp far into the night on several occasions, under hazardous circumstances. However, by any other procedure, time would not have been adequate to complete the regional survey.

The stratigraphic section measured around the lake is, in the aggregate, enormous, measuring in the order of 65,000 feet. This includes Lower and Middle Devonian, Lower Permian, Upper Jurassic, Middle and Upper Cretaceous, and Tertiary rocks. Approximately one-half of the entire column studied belongs to the Tertiary, which consists of non-marine sediments and volcanics.

The most noteworthy general features of the geologic structure are several basins, separated by uplifts, or massifs, and numerous large faults, both normal faults and low-angle overthrusts. The majority of these principal structural features were formed during and before the last great compression of the Andes, probably no later than the Miocene.

The salient features of the geological history were determined by: (1) studies of the rocks themselves, with their enclosed fossils; and (2) the character and distribution of unconformities which shed light on the times of orogeny.

After the Middle Devonian seas were expelled from the region, by uplift, the area was probably subjected to erosion until encroachment by the Early Permian seas from the northward. During this great span of geologic time, there was negligible warping of the area in Bolivia and southern Peru.

It is uncertain whether or not the area was folded at the close of the Paleozoic. There was considerable volcanic activity during the Lower Permian in central Peru, but Permian and Triassic strata are essentially parallel in Argentina and at least locally in central Peru. In the Titicaca area, the youngest Paleozoic (Lower Permian) and the oldest Mesozoic (Upper Jurassic) do not occur in the same areas.

Nevertheless, the Jurassic rests with angular relations on tilted Devonian, and elsewhere the Devonian and Permian are parallel. An orogeny is indicated somewhere within the span of this interval, between the Devonian and Jurassic. It may have been one of the Hercynian stages. Angular relations between the Middle Cretaceous and Upper Jurassic rocks suggest a Nevadian orogeny. These Middle Cretaceous rocks truncate across all older rocks. Evidently, this was one of the most significant orogenic cycles in the history of the Central Andes.

The overlap of Upper Cretaceous rocks onto older rocks in the Eastern Cordillera indicates some activity, in that region, near the beginning of the Senonian.

No Laramide orogeny has thus far been recognized in the eastern Andes of Argentina, Bolivia, and Peru. However, the Flysch-like Early Tertiary deposits, of enormous thickness, in the Altiplano, truncate all older rocks. Their coarse texture implies deposition in a region of high relief, in which depositional basins were continuously sinking to receive sediments. Abundantly contained volcanic materials indicate active volcanism, probably in the Western Cordillera.

The development of the modern Andes, since the beginning of the Tertiary, may be divided into a series of stages. There is no general agreement regarding the sequence of events, nor their dating. Our information is no more complete than it was for the Rocky Mountains sixty years ago.

In the Early Tertiary, intermontane or Piedmont basins were filled with thousands of meters of coarse red arkosic beds, containing gypsiferous shales toward the top (Puno group). These beds were disturbed (Miocene?), and the folds were truncated by erosion.

Then came the third great period of volcanism in the Western Cordillera (Tacaza), following the earlier volcanism of the Early Jurassic (Chocolate) and Early Tertiary (Puno). The third stage of volcanism resulted in great piles of bedded volcanics, locally attaining more than 3000 meters in thickness.

The last orogenic cycle of the Central Andes followed. The region was broken by great folds and overthrusts, which were shoved from opposite directions on the two sides of Titicaca. There is no conclusive fossil evidence which can be used for the dating of this orogenic cycle. None the less, it could hardly have occurred earlier than Pliocene. Such paleontologic evidence as is available indicates a Late Tertiary date.

The great upland Puna peneplain of the Andes was formed after this orogeny. Since the erosion surface truncates the great structures with amazing evenness, it would seem to indicate a long epoch of erosion, resulting in late mature topography not far above sea level.

The great volcanic blanket of the Western Cordillera, measuring up to hundreds of meters in thickness, rests on this old peneplain. Likewise, the basin of the Altiplano is clearly a younger topographic and structural feature, formed by the down-warping and collapse of the old Puna surface. The first lake to occupy the basin was clearly pre-Glacial. It may or may not have lasted into the Pleistocene. It is not certain that the Andes had yet risen high enough to be glaciated during the earliest stage of the Pleistocene. The ancestor of Titicaca (Lake Ballivián) left high water marks in the form of wave marks and lacustrine deposits up to 100 meters or more above the present level of Titicaca.

The water level dropped to its present elevation by downcutting of the outlet, Rio Desaguadero. The configuration of the present floor of Lake Titicaca suggests quite recent activity along the northeast margin, producing a very deep and youthful basin, in which sediments have only just begun to level the bottom.

The latest subsidence in the level of Lake Titicaca, still in progress, seems to be correlated with increasing aridity of the region and recession of the glaciers in the surrounding ranges.

SECTION OF BIOLOGY

OCTOBER 14, 1946

DOCTOR LOUIS M. PRUESS, Research Department, Lederle Laboratories Division of American Cyanamid Company, Pearl River, New York: *Streptomycin.*

In preparing this paper, I have purposely omitted most of the details of the topic. Instead, I have confined myself to a general presentation or summary of the origin, nature, properties, clinical use, and limitations of streptomycin.

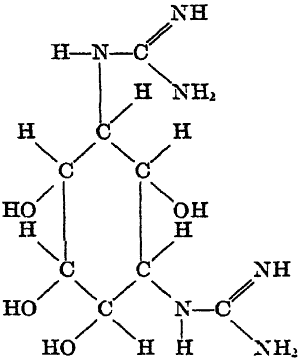
The story of penicillin is a familiar one. As will be recalled, its discovery by Fleming was, in a sense, accidental. The story of streptomycin, however, is entirely different. This antibiotic substance was discovered, not by chance, but as the result of a deliberate search for a product capable of exerting a bacteriostatic or bactericidal effect upon gram-negative bacteria, and not only active against these organisms in the test tube but also in the animal body, yet not very toxic. In other words, it was a planned search for a substance offering chemotherapeutic potentialities. Professor Selman Waksman and his collaborators of the New Jersey Agricultural Experiment Station, at Rutgers University, reported in January, 1944, that they had found such an agent. They called it streptomycin, the name being derived from the generic designation of the organism producing the substance, a sporulating and aerial-mycelium-forming species of the Actinomycetes, *Streptomyces griseus.* The characteristics of this organism relate it to both bacteria and molds. Two cultures of the streptomycin-producing strain of *Streptomyces griseus* were obtained, one from a heavily manured field soil and the other, a somewhat less active form, from the throat of a chicken.

Streptomycin can be produced in both natural and synthetic media, in submerged as well as surface culture. Waksman's group originally adopted a medium consisting of 1% glucose-0.5% peptone-0.5% meat extract-0.5% NaCl, pH 6.5–7.0. It was soon found that the nitrogen and carbon sources in the medium are more or less non-specific, and such cheaper materials as corn steep liquor, soy-bean meal, milk and yeast by-products, or known chemical compounds in the form of synthetic or semi-synthetic media could replace the more expensive peptone and meat extract of Waksman's original medium. The yield of streptomycin, of course, depends upon the strain, medium, fermentation conditions, and recovery process used in its production. In submerged culture, maximum growth usually occurs in two days, accompanied in one to three days by maximum potency. In stationary culture, the maximum activity is usually reached at a somewhat later time, seven to ten days. Temperatures between 25–30° C. have been found to be optimum. Although the organism grows well at temperatures higher than 30°, streptomycin formation decreases, and at temperatures lower than 25° it is retarded. Strains of *Streptomyces griseus* that produce streptomycin may undergo variation. Inactive variants isolated from such cultures are characterized by the fact that they are sensitive to the antibiotic action of streptomycin, produce no aerial mycelium, form no streptomycin, undergo rapid lysis, give rise to an acid reaction, and yield a viscous broth.

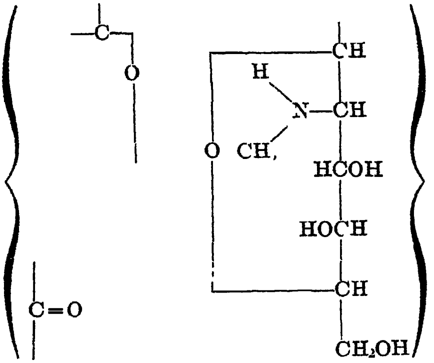
Streptomycin is an organic base soluble in water and insoluble in organic solvents. Concentrates can be prepared as follows. The mass of growth of the organism is separated from the acidified culture medium, by filtration. The selection of a filter and filter aid is important, since streptomycin is adsorbed on some materials. The cell-free culture fluid is then made alkaline and treated with active carbon, which adsorbs the antibiotic. The carbon is filtered off, washed with water or alcohol to remove some of the impurities, and the streptomycin is then usually eluted with acid methanol. After removal of the carbon, the acid eluate is neutralized and then filtered to remove impurities that may have precipitated out. The resulting filtrate is evaporated *in vacuo* to a concentrate. A solid preparation may be obtained by precipitation of such a concentrate with ether or acetone, or by desiccation *in vacuo*. From such concentrates, *via* the picrate salt and subsequent chromatography, three crystalline salts of streptomycin have been prepared. One is the reineckate sulfate, a double salt, which crystallizes from water in thin plates and decomposes at 162–164° C. Another is the helianthate, which is recrystallized from aqueous methanol. The anhydrous helianthate darkens at 205° and melts with decomposition at 220–226°. The third is streptomycin trihydrochloride-calcium chloride. This colorless double salt crystallizes from alcohol as needles, and decomposes between 200–230°. All these salts can be converted to streptomycin hydrochloride or the sulfate, the commonly dispensed forms. From various analytical data, the present formula of streptomycin is given as $C_{21}H_{39}N_7O_{12}$. Without going into any of the details, I will merely state that the experimental results, thus far, indicate that streptomycin has the following general constitution:

$C_{21}H_{39}N_7O_{12} + H_2O = C_8H_{18}N_6O_4 + C_{13}H_{23}NO_9$
(Streptomycin) (Streptidine) (Streptobiosamine)

$C_{13}H_{23}NO_9 + H_2O = C_6H_{10}O_5 + C_7H_{15}NO_5$
(Streptobiosamine) (Streptonose or Streptose) (N-methyl-alpha-levo-glucosamine)



(Streptidine)



(Streptonose or Streptose) (N-methyl-alpha-levo-glucosamine)
(Streptobiosamine)

Streptomycin is completely inactivated on standing in acid solution, undergoing hydrolytic cleavage into two basic fractions which have been designated as streptidine and streptobiosamine. The streptidine, 2,4,5,6-tetrahydroxy-1,3-diguanido-cyclohexane, is attached through a glycosidic linkage to the nitrogen-containing, disaccharide-like streptobiosamine molecule. The latter half of the streptomycin molecule is composed of N-methyl-alpha-levo-glucosamine, the carbonyl group of which is glycosidically joined to a moiety called streptonose or streptose, and which contains a free or potential carbonyl group. This carbonyl group is changed to an alcohol group when streptomycin is catalytically hydrogenated to yield dihydrostreptomycin. The latter compound has practically the same antibacterial activity as streptomycin. Unlike streptomycin, the dihydro derivative was found to be stable in alkaline solution, and it was not inactivated by hydroxylamine, semicarbazide, or cysteine. Where the linkages are in the streptomycin molecule, and what is the constitution of the streptonose or streptose moiety of the streptobiosamine half, remain to be determined for a complete picture of the structure of streptomycin.*

In contrast to penicillin, streptomycin is markedly stable. Dilute sterile solutions have maintained their initial activity after fifteen to seventeen days at 37° C. Heating solutions of streptomycin for ten minutes at 100° C. produces an inactivation of less than 50 per cent. The substance is not destroyed by microorganisms, as are such antibiotics as penicillin, gliotoxin, and pyocyanase. Waksman has reported that when streptomycin solutions, prepared on the one hand with water and on the other with nutrient broth, were inoculated with *Aspergillus niger* or with a mixed culture of bacteria and fungi, the activity of the cultures, after incubation at 28° C. for seven days, was no less than that of the sterile controls. Streptomycin may be reversibly inactivated by cysteine. A less active agent is thioglycollic acid. However, this inactivation is not a property of the sulfhydryl group alone, nor is it limited to organic sulfur compounds, since inorganic reducing agents, as well as ascorbic acid and ketone reagents, also antagonize the antibacterial activity of streptomycin. Whether this antagonism by reducing agents is due to a lowering of oxygen tension, a reduction of the oxidation-reduction potential of the medium, or to some specific chemical reaction, such as the blocking of an active grouping in the streptomycin molecule, or to all of these acting simultaneously, is still unknown. It is entirely possible that the phenomenon is related to the mode of action of streptomycin. This antibiotic displays its greatest activity against bacteria which grow better aerobically than anaerobically; almost all the obligate anaerobes are not susceptible. The antibacterial action of streptomycin may be due to its ability to block some enzyme system, oxidative in nature, which is essential only to the growth of susceptible aerobic bacteria, an enzyme system which anaerobes do not possess. It is conceivable that, in certain parts of the body where reducing agents are present or where a low oxygen tension exists, larger concentrations of streptomycin may be necessary to inhibit the growth of bacteria.

It is evident that the choice of a medium is important in assaying streptomycin and in determining the susceptibility of bacteria to its antibacterial activity. The presence of glucose tends to reduce the activity of streptomycin appreciably in certain media with some, but not all, organisms. This may be due to the reducing properties of glucose, or to the production of some acid by the test organism. Infusion medium, because of its high content of reducing agents, would also be an unwise choice for assay purposes. A low salt content and a pH of about 8.0 are also preferred. The pH effect may be due to the fact that streptomycin, being an organic base, is not so active in the form of a salt as in a free basic state, or that the active part of the molecule is neutralized by acid.

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* See note at end of the paper

Several methods are commonly used for assaying streptomycin, namely, the liquid serial dilution method, the agar plate dilution method, the cup-plate or agar diffusion method, turbidimetric, polarographic, and other procedures. For most quantitative measurements, the agar diffusion method is usually used, with *Bacillus subtilis* as the test organism. Paper discs or small cylinders are employed to contain the test solution, which diffuses into the agar, seeded with the assay organism, to produce circular zones of growth inhibition. The activity of the unknown preparation is compared with the inhibition of growth produced by a given standard, and the potency is calculated from the comparative values obtained. Since normal blood contains a diffusible agent which produces large zones with *Bacillus subtilis* as the test organism, a particular strain, SM, of *Staphylococcus aureus* is used in the assay of blood. A turbidimetric method in which a noncapsulated strain of *Klebsiella pneumoniae* is used as the assay organism has also found acceptance for the testing of streptomycin samples. In pharmacological and clinical work, streptomycin can also be quantitatively determined by the slide cell method, similar to that used for assaying penicillin in body fluids; the principal change is the substitution of *Bacterium megatherium* for *Streptococcus pyogenes* as the test organism.

Originally, a unit of streptomycin, or an S unit, was arbitrarily defined as that amount of material which, when present in 1 ml. of nutrient medium, will just inhibit the growth of a given strain of *Escherichia coli.* This unit is a convenient quantitative standard for the study of the production and isolation of streptomycin, as well as for microbiological studies of its antibacterial action *in vitro.* The small size of this unit, however, was rather awkward for pharmacological investigations and clinical use. Consequently, Waksman proposed, in addition to the S unit, an L unit, or that amount of material that will just inhibit growth of a particular strain of *Escherichia coli* in 1 liter of nutrient medium, and a G unit based on the activity of the pure material. For the pure streptomycin base, a G unit equals 1,000 L units or 1,000,000 S units, and on a weight basis, one microgram is equivalent to one unit. It is of importance that the unit size of streptomycin be clearly understood, especially when attempts are made to compare the efficacy of this substance with that of penicillin, since the two antibiotics are standardized differently. Whereas the Oxford unit of penicillin is based on inhibition of the gram-positive *Staphylococcus aureus* in 50 ml. of medium, the streptomycin unit is defined in terms of its action against the gram-negative *Escherichia coli* in 1 ml. of medium. The Oxford unit represents a much greater *in vitro* activity than does the streptomycin unit. In terms of dry weight, 1 streptomycin unit equals 1 microgram, whereas 1 Oxford unit of penicillin equals 0.6 microgram of penicillin G.

Tissue culture studies, animal experiments, and clinical use have demonstrated that, while its margin of safety is definitely higher than that of all chemotherapeutic agents other than possibly penicillin, streptomycin, at least that which has been used to date, has definite toxic properties. Both the commercial product and the crystalline streptomycin as produced at present will cause untoward reactions in a certain number of patients. The frequency of reactions increases with the size of the dose and with the duration of the treatment. Local reactions such as pain, soreness, and induration at the site of injection are frequent, but become a less serious problem with more purified preparations. Nausea, vomiting, headache, facial flushing, fall in blood pressure, and fainting were seen in some patients and may be ascribed to a histamine-like impurity. Some of the early preparations of streptomycin contained small amounts of such impurities. Pure streptomycin is free from these side-effects. At present, no sample of streptomycin is passed by the producer or the Food and Drug Administration, unless, when injected intravenously into cats on the basis of 300 micrograms per kilogram of body weight, it causes no greater fall in blood pressure than the equivalent of 0.1 microgram of histamine base. Other reactions, such as toxic erythema and cutaneous rash with or without fever and eosinophilia, may occur at times after several days of administering streptomycin. There is nothing specific about the type of skin reaction. It may be caused by abnormal sensitivity of the patient or by some impurities in the streptomycin preparation. The most serious types of toxicity have been observed primarily in manifestations of involvement of the eighth cranial nerve. Tinnitus and vertigo are usually the first symptoms noted. Both auditory and vestibular functions have been definitely reduced in some patients, particularly in those who received large amounts of streptomycin over prolonged periods. It is not known whether these neurologic disturbances are due to streptomycin or to small amounts of impurities, but all the available evidence suggests that streptomycin itself is at fault. Some renal irritations, manifested by a transient increase in urinary output, hematuria, and the presence of hyaline and granular casts in the urine, have been noted following the administration of large doses of streptomycin. Fatty metamorphosis of the liver and kidney has been observed in animals. However, careful studies on man have failed to reveal any evidence of significant hepatic or renal damage from pure preparations of the drug.

Streptomycin has been administered intramuscularly, intravenously, and subcutaneously, both by intermittent injection every three or four hours, and by continuous drip. It has also been given by the oral route, by nebulization, and by the intrathecal route. For systemic treatment, the intramuscular route is the one of choice. Following parenteral injection, streptomycin is excreted largely by the kidneys into the urine, but the drug was found to be distributed throughout most body fluids, such as ascitic, pleural, peritoneal, bile, aqueous and vitreous humors, and it has been found to enter fetal cord blood and amniotic fluid. When given orally, only occasionally was there any streptomycin in the blood or urine, and then in small amounts. Most of the drug was eliminated in the feces. Apparently, streptomycin passes through the gastrointestinal wall with difficulty, since little appears in the blood when given orally, and only small amounts are found in the gastrointestinal tract when given intravenously (that which is found probably entering from the bile). This persistence and stability in the gastrointestinal tract has been used to advantage in preparing patients for operations on the colon, and for the treatment of typhoid patients and carriers. Oral administration rendered the feces completely negative for *Eberthella typhosa* and considerably reduced the numbers of *Escherichia coli* as well as the general fecal population. Streptomycin administered in peanut oil and beeswax is not absorbed from the muscles as readily as when it is injected as a saline solution; lower serum levels and reduced urinary excretion were obtained with the former method than with the latter. However, absorption of the compound emulsified in "solvecillin" is similar to that following saline solution injections.

Considerable data are being collected on the clinical value of streptomycin. Evidence is accumulating as to the effectiveness of this agent in the treatment of a limited number of infections and its ineffectiveness in the treatment of many others. Streptomycin has been found most effective in tularemia, in meningitis due to *Hemophilus influenzae* infection, in bacteremias and meningitis due to gram-negative bacteria, and in enteric infections. Some heretofore resistant genito-urinary tract infections respond well to treatment with streptomycin, but the results in the routine treatment of urinary infections with streptomycin are disappointing. It is undoubtedly a very useful agent in this type of infection if the causative bacteria are susceptible and if there is a free flow of urine. Encouraging results have been obtained in the treatment of certain types of pulmonary disease, especially if streptomycin is combined with penicillin therapy. Nebulization therapy was effective in providing a reduction of the volume of sputum in patients who had non-surgical, chronic bronchiectasis. Streptomycin proved of value in some cases of peritonitis and warrants a more extensive clinical study. The results in typhoid, brucellosis, cholera, *Salmonella* infections, and wound infections have been inconclusive and disappointing. Although streptomycin has been shown to check and retard the normal course of experimental tuberculosis in guinea pigs, no such clear-cut evidence was obtained for this disease in human patients. It had a palliative effect and was valuable as an adjunct when combined with other established methods of treating this infection. The practical use of this antibiotic in pulmonary tuberculosis must await further study, as soon as adequate supplies become available. However, a case of laryngeal tuberculosis, with only slight pulmonary involvement, and a case of bladder tuberculosis were reported cured by streptomycin.

As has been found with the sulfonamides and penicillin, the sensitivity of a given culture of an organism to streptomycin is characteristic not only of the organism but of the strain as well, different strains varying greatly in this respect. Also, some organisms at times develop a resistance to streptomycin with incredible rapidity. Thus, for example, tubercle bacilli, gonococci, and meningococci have been cultivated whose resistance to streptomycin has been increased a thousand and more times in a short interval. *Salmonella, Shigella, Proteus, Aerobacter*, and *Escherichia* cultures have also been made resistant. This drug resistance is a matter of concern among clinicians and epidemiologists. Fortunately, the resistant or adapted variants may not be simultaneously resistant to other drugs and antibiotics. The relation of artificially acquired and naturally acquired resistance to chemotherapeutic agents involves many questions as yet unanswered.

There may be a number of reasons for the failure of streptomycin, or, for that matter, of any other antibiotic therapy. The following five conditions probably account for most failures:

(1) Treatment of infections that are not susceptible to the action of streptomycin: that is, the organism is so resistant that a concentration of streptomycin adequate to inhibit the growth of the organism cannot be maintained in the blood or tissues.

(2) Inadequate dosage may be used, even in the presence of a susceptible organism. It is essential that bacteria be eradicated in the shortest possible time if satisfactory clinical results are to be obtained. This means the administration of large doses from the onset of treatment.

(3) Development of resistance to streptomycin. I have already mentioned this. Resistance appears to take place particularly in bacteremia, and urinary tract infections.

(4) There may be a change in the species of infecting organism during treatment. In urinary and pulmonary infections, which frequently are characteristically polyvalent, the sensitive or susceptible organisms or strains are eliminated or decreased in numbers, and nonsensitive organisms grow and multiply and invade tissues. The change in the bacterial flora of urine and sputum after streptomycin treatment may be very striking.

(5) There may be a localization of infection in an area that cannot be reached by streptomycin. Foreign bodies and tumors should be surgically removed as early as possible. It has been suggested that the reason why streptomycin has proved of so little value in typhoid and brucellosis may be that the causative organisms are found intracellularly, the antibiotic thus being unable to reach them, even though adequate amounts of the drug appear in the blood during therapy.

In conclusion, I would like to add on September 1, 1946, the Civilian Production Administration announced the names of 1,652 hospitals in the United States and the Territories which will act as depots in the controlled distribution of streptomycin for civilian treatment. Each depot hospital is allotted a specific quantity of streptomycin available during the month. Any hospital or physician may apply to the nearest depot hospital for needed supplies.* Previous to September 1, grants-in-aid for clinical study of streptomycin, amounting to almost one million dollars, were contributed in equal shares to the National Research Council by eleven pharmaceutical manufacturers constituting the Streptomycin Producers Advisory Committee of the Civilian Production Administration. Dr. Chester S. Keefer, who headed the clinical investigation of penicillin for the Office of Scientific Research and Development during the recent war, was in charge of the clinical trials program.

Total U. S. streptomycin production has increased from approximately 3,000 grams in September, 1945, to about 150,000 grams in September, 1946. Of course, not all of this is available for civilian use, because military and other government agencies are also allocated supplies. One to four grams of streptomycin a day for five to fourteen days are usually required in the treatment of infections other than tuberculosis. Large-scale production by several manufacturers soon will not only increase the available supply of streptomycin, but will also lower the cost of this antibiotic, just as was the case with penicillin.

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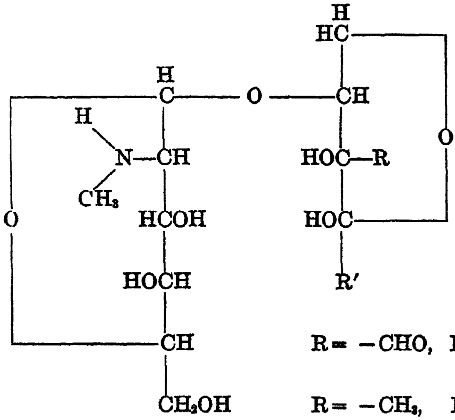
* The C. P. A. has recently suspended its quota system for distributing streptomycin. Hospitals and sanatoriums may now order the drug direct from suppliers, without restriction. However, the amount of the drug which a manufacturer may release is restricted by allocation. No provision is made for distribution through ordinary drug channels, or to physicians individually. Further relaxation of controls has been recommended.

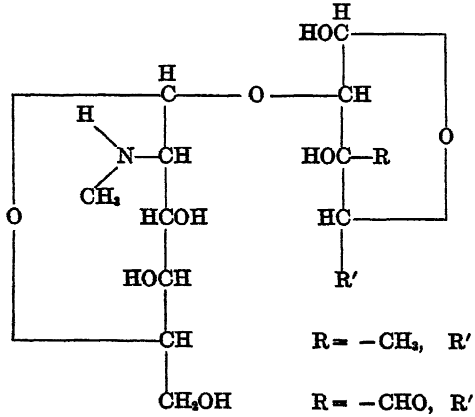
NOTE ADDED IN PROOF, NOV. 25.

One of the following four formulas has recently been proposed by investigators at Merck and Co., Inc., as representing the structure of streptobiosamine:



$R = -CHO$, $R' = -CH_3$ or

$R = -CH_3$, $R' = -CHO$



$R = -CH_3$, $R' = -CHO$ or

$R = -CHO$, $R' = -CH_3$

SECTION OF PSYCHOLOGY

OCTOBER 21, 1946

DOCTORS ALICE I. BRYAN, School of Library Service, Columbia University, New York, N. Y., and EDWIN G. BORING, Department of Psychology, Harvard University, Cambridge, Massachusetts: *Women in American Psychology: Factors Affecting Their Careers.**

To obtain the material reported in this article, we sent a questionnaire to each of the 440 American women who took Ph.D.'s in psychology between 1921 and 1940, and also to each of 440 men, matched individually to the women by university and date of Ph.D. The questionnaire dealt with matters that affect the professional careers of psychologists. We received replies from 245 women, which is about 60 per cent of the corrected population, and from 247 men, which is probably about 30 per cent of the corresponding male population. The replies are dated from December, 1945, to April, 1946.

RESULTS

(1) The women Ph.D.'s appear to have had family backgrounds more favorable to advanced education than the men. The women's parents have had, on the average, more education than the men's. The women have fewer fathers who did not get through high school, more fathers who took advanced degrees after college. The women have fewer foreign-born parents. Jobs of the women's fathers lie higher in the occupational scale than the men's, and more of the women's mothers have had professional positions than those of the men's.

(2) The average picture of how these persons decided to become psychologists would be that they became interested in psychology in their late teens, decided on a career in psychology at about 24, and got their Ph.D. at about 29.

(3) These psychologists, in general, expressed satisfaction with their professional training, and then went on to complain that they had not had more of it. In a list of 25 subjects in which graduate students can be trained, our 492 persons checked 1848 subjects as fields in which they wished they had had more training, and only 408 items as having been emphasized too much in training. This criticism reflects the shift in psychology away from the laboratory and old-

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* The lecture was delivered by Doctor Bryan.

fashioned scholarship toward clinical practice and personnel work. Both men and women would have liked more training in clinical techniques, personnel procedures, field work, internships, and statistics, and less emphasis upon foreign languages (especially German), experimental psychology, laboratory practice, systematic psychology, and the history of psychology. Sex differences in these wishes are not striking.

(4) We sought to assess the prevalence of prejudice as affecting employment. Employers have a fear of incompetence in general, sometimes directed against youth, in particular, and sometimes against age. Only little prejudice against political views and activities is reported, and not much more against race and religion. Both men and women are convinced that there is much more prejudice against the female sex (and practically none against the male sex), but this must be considered in relation to the other fact that, in some psychological work, it is helpful to be a woman. Within professional psychology, 'woman's work' and 'man's work' are not always the same (see also item (9) below).

(5) Seven men in ten, six women in ten, have full-time employment in psychology. The men, taken all together, spend 82 per cent of their time in paid psychological employment, the women 65 per cent of theirs. In all kinds of paid employment, the men spend 98 per cent of their time, the women 72 per cent. Responsibilities of the women for household and children are not counted as paid employment.

(6) Women get lower salaries than men, and married women slightly lower salaries than unmarried ones, even when all the persons considered have full-time paid employment in psychology. Salaries tend to increase with age. We think the fact that men in their thirties have recently been doing as well financially as men in their forties to be an artifact of war conditions. Although some of our results are affected by the depreciation of the dollar in the last quarter-century, we believe it to be approximately true that, at the present time, women psychologists, when fully employed, whether married or unmarried, make on the average about 20 to 40 per cent less than men of the same ages, being paid from $1,000 to $2,000 a year less.

(7) Most of the psychologists who were questioned like their profession. Hardly any of them regret what he or she has done within or for the profession. There are slightly more complaints about the present job than about the profession, but not many. The psychologists know how to accept the reality which is theirs.

(8) The women seem almost as satisfied with their profession and their professional activities as the men, although they make it clear that marriage and profession are often in conflict. For many married women in psychology, responsibility to the family becomes more important than professional work. In the great majority of cases, the women accept this potential conflict situation with equanimity, concurrently deriving satisfactions from husband, children, and work.

(9) Both men and women think that women, on the average, are professionally at a disadvantage because of their sex. The men get work more easily. They earn more when they get it. They achieve more, because they lack the women's conflict in marriage and thus get promoted more readily. On the other hand, at least one-fourth of the women claim that their sex has made it easier for them to secure desired work. Presumably, they are the women with the women's jobs in psychology, the jobs in the schools, educational systems, clinics, guidance centers, hospitals, and custodial institutions (cf. item (16) below).

(10) Both married women and married men like their marriages. We tried to give ample opportunity for complaint about marriage, and received complaints, but not in sufficient number to make us believe that there is much error in the direct statements that marriage is fairly satisfying or very satisfying to about 90 per cent of both groups.

(11) On the other hand, the question as to whether marriage helps a professional career brings out a clear sex difference. For men, marriage helps in three-fourths of the cases, while the rate for women is only a little more than one-third. A quarter of the women with full-time employment in psychology say that marriage makes a career more difficult, whereas more than half of the women with part-time employment in psychology either check the same statement or else note that marriage has caused them actually to abandon a career.

(12) Children affect careers much as does marriage. Having children interferes with a woman's career more than a childless marriage, and it is also of less advantage to a man in his profession.

(13) The women and the men show no sex differences in respect of the number of professional societies to which they belong, the number of offices and committee chairmanships which they hold, and the number of meetings which they attend every year.

(14) Men and women with full-time employment in psychology do more professional reading than those with part-time employment.

Married women read, on the average, less professional literature than unmarried women, and unmarried women a little less than men.

(15) The married and unmarried women with full-time employment in psychology spend, on the average, about the same amount of time in research and writing, but very much less time than the men.

(16) We think that items (14) and (15) are to be explained in relation to item (9), which in turn accounts for item (6). There is a sex difference between the interests of women and men, perhaps imposed by the culture. At any rate, it exists, and women view it realistically, accept it, and learn to like it. The women prefer the particular and the personal. The men like the theoretical and the general. All interest inventories bring out this sex difference. Thus, the women like clinical practice, personnel work, and guidance, and do not write about it, because it is personal. The men like research and writing. The men read more than the women, because research and writing are associated with reading, but the women also read more than their research and writing would have indicated. Since research and writing, and the large generalizations that research yields and that are most appropriately written about, all lead to renown, the male psychologists become more famous than the female, and then receive larger salaries, partly for the reason that fame has cash value.

(17) The women in our group spend more time on domestic and familial activities than do the men, but the men spend a good deal of time in this way, mostly on the care of property. About one man in five reports doing some general housework, though none claims responsibility for sewing and mending.

(18) Seven or eight hours a week is all the time that our psychologists average on recreational activities. There is no clear sex difference here. Also, our group contains few "social butterflies," either male or female.

(19) The women average about four weeks' vacation a year, the men about three. The three-month summer vacation, which all academics were once supposed to have, seems to have disappeared from the calendars of the psychologists.

CONCLUSION

There is nothing new in the statement that a woman is handicapped in a professional career by the common belief that a woman's place is in the home. Our way of life precludes her free concentration on a profession. If she marries, her marriage conflicts with her professional work, while if she has children, the conflict is even more severe, unless she gives up much of her professional work. If she is unmarried, she still has, as a rule, to pay more attention to domestic matters than does an unmarried man, and when she marries, her husband seldom takes over the household responsibilities in the way a wife is expected to do, nor does her husband often become her professional assistant corresponding to the manner of many wives.

Women face this inescapable conflict and make the necessary compromises. Some of them hold to their careers and do not marry. Some marry and maintain full-time professional work, remaining, however, at a disadvantage in relation to their male competitors. Other women drop back into part-time professional work, or, abandoning their careers, into little or no professional work at all.

We find that most of our women Ph.D.'s in psychology have accepted this reality and are not protesting, or at least not vigorously, about what they recognize as a necessity of living in our culture. They like marriage. They like their profession. They would, they say, repeat their lives without essential change if they had to live them over. They recognize the disadvantages under which they operate, but they also appreciate the advantages of marriage Some of them, to repeat, find their sex actually a professional advantage, because there is such a thing as 'women's work' in psychology, and women are wanted for it, prefer it, and do well in it. It seems not to be so well paid as some other kinds of professional activity.

This specific focusing of interest is an important fact about women. In addition to being the acknowledged home-builders, women constitute a group especially concerned with personal and particular matters. For the most part, they like to work with individuals and not to attempt the abstract generalizations. The men, in general, undertake more research and writing and also a little more professional reading. In this way, the women fall behind in the race for fame and also in the race for salary. Most of them seem to accept this reality as it comes to them, to get accustomed to it and like it. They form, on the whole, a very well adjusted group, well-poised, not militant, and yet, when marriage does not interfere, professionally quite as active as the men. We have come to hope that this finding, which surprised both authors, may mean that women psychologists know how to use their own therapy upon themselves, accepting and even learning to like the inevitable, while remaining intelligently aggressive toward reform in many other spheres.

SECTION OF ANTHROPOLOGY

October 28, 1946

Doctor W. Duncan Strong, Department of Anthropology, Columbia University, New York, N. Y.: *Excavations and Discoveries in Viru Valley of Northern Peru.* (This lecture was illustrated by Kodachrome slides.)

(No abstract received.)

GENERAL CONFERENCE

OCTOBER 21 AND 22, 1946

Conference on "*Teleological Mechanisms.*"

The New York Academy of Sciences held a Conference on "Teleological Mechanisms." Doctor Gregory Bateson, John Simon Guggenheim Fellow, was the Organizing Chairman in charge of the meeting.

The program consisted of the following papers:

MONDAY, OCTOBER 21

Morning Session. Chairman, L. K. Frank.

"Self-Correcting and Goal-Seeking Devices and Their Breakdown," by Norbert Wiener, Massachusetts Institute of Technology, Cambridge, Massachusetts.

"The Control of Movements of Animal Organisms," by Arturo Rosenblueth, Institute of Cardiology, Mexico City, Mexico.

Afternoon Session. Chairman, Gerhardt von Bonin.

"Circular Causal Mechanisms in Ecology," by G. E. Hutchinson, Osborn Zoological Laboratory, Yale University, New Haven, Connecticut.

"Circular Causal Systems in Society," by Gregory Bateson, John Simon Guggenheim Fellow.

TUESDAY, OCTOBER 22

Morning Session. Chairman, Harold G. Wolff.

"The Vicious Circle in Causalgia," by W. K. Livingston, M.D., Captain, U.S.N.R.

"Circular Processes in Public Opinion," by Paul Lazarsfeld, Department of Sociology, Columbia University, New York, N. Y.

Afternoon Session. Chairman, H. Brosen.

"The Logic of Signs and Signals in Computing Machines," by John von Neumann, Institute of Higher Learning, Princeton, New Jersey.

"Summary of Theory and Extension to Gestalt Psychology," by Warren S. McCulloch, Neuropsychiatric Institute, University of Illinois, Chicago, Illinois.

SECTION OF BIOLOGY

OCTOBER 31 TO NOVEMBER 2, 1946

Conference on *"Hemorrhage."*

The Section of Biology held a Conference on "Hemorrhage." Doctor Gregory Shwartzman, The Mount Sinai Hospital, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

THURSDAY, OCTOBER 31

"Introductory Remarks," by the Conference Chairman.

Biochemical Aspects. Chairman, Charles H. Best.

"Some Basic Facts of Coagulation," by John H. Ferguson, University of North Carolina, Chapel Hill, N. C.

"Heparin in Blood Coagulation and in Thrombosis," by Charles H. Best, University of Toronto, Toronto, Canada, and Louis Jacques, University of Saskatchewan, Saskatoon, Canada.

"The Relationship of Vitamin K to Hemorrhage and Coagulation," by S. A. Thayer, School of Medicine, St. Louis University, St. Louis, Missouri.

"Hemorrhagic Manifestations Observed in Experimental Deficiency of Pantothenic Acid, Choline, and Cystine," by Paul György, University of Pennsylvania Medical School, Philadelphia, Pennsylvania.

Physiological Aspects. Chairman, Donald D. Van Slyke.

"The Effects of Hemorrhagic Shock on the Circulation," by Dickinson W. Richards, Jr., College of Physicians and Surgeons, Columbia University, and First Medical Division, Bellevue Hospital, New York, N. Y.

FRIDAY, NOVEMBER 1

"Experimental Studies on Traumatic and Hemorrhagic Shock," by Magnus I. Gregersen, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"The Capillary Circulation under Normal Conditions and in Acute Hemorrhage," by Robert Chambers and Benjamin W. Zweifach, Washington Square College, New York University, New York, N. Y.

"The Capillary Circulation in Hemorrhagic Shock," by Benjamin W. Zweifach, M. I. Kopac, Richard Lee, and Robert Chambers, Washington Square College, New York University, New York, N. Y.

"The Influence of Humoral Factors of Hepato-Renal Origin on the Vascular Reactions to Hemorrhage," by Ephraim Shorr, Benjamin W. Zweifach, and Robert F. Furchgott, Department of Medicine, The New York Hospital, New York, N. Y.

"Effects of Hemorrhagic and Traumatic Shock on the Kidney," by Donald D. Van Slyke, Hospital of The Rockefeller Institute for Medical Research, New York, N. Y.

"Metabolic Changes Associated with Hemorrhage," by A. E. Wilhelmi and C. N. Hugh Long, Yale University, New Haven, Connecticut.

"The Significance of Blood Volume Alterations in Traumatic Shock," by Everett Idris Evans, Medical College of Virginia, Richmond, Virginia.

Evening Session: Motion Pictures

(1) Functional Activity of the Muscular Vessels in the Capillary Bed (black and white);

(2) Effect of Anesthesia on Peripheral Circulation Following Hemorrhage (colored), shown by Robert Chambers and Benjamin W. Zweifach.

SATURDAY, NOVEMBER 2

Pathological and Clinical Aspects. Chairman, George R. Minot.

"Certain Anatomo-Pathological Aspects of Hemorrhage," by Paul Klemperer, The Mount Sinai Hospital, New York, N. Y.

"Hemorrhagic Manifestations of Bacterial and Virus Infections: Experimental Studies and Pathological Interpretations," by Gregory Shwartzman and I. E. Gerber, The Mount Sinai Hospital, New York, N. Y.

"Abnormal Hemorrhage with Normal Platelets and Normal Clotting Time," by Russell L. Haden, Cleveland Clinic, Cleveland, Ohio.

"Hemophilia," by George R. Minot, Harvard Medical School and Boston City Hospital, Boston, Massachusetts.

"The Manifestations and Management of Purpura Hemorrhagica," by Joseph C. Turner and R. H. Egerton Elliott, Presbyterian Hospital, and College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Clinical Aspects of Hypoprothrombinemia," by Charles S. Davidson and Henry I. Tagnon, Harvard Medical School, Boston, Massachusetts.

NEW MEMBERS

A. ELECTED FROM MAY 15 TO SEPTEMBER 13, 1946

LIFE MEMBERSHIP

Price, Jerry C., M.D., Cause, Diagnosis and Treatment of Epilepsy. Research Associate in Neurology, College of Physicians and Surgeons, Columbia University, New York, N. Y.

SUSTAINING MEMBERSHIP

Gottlieb, Charles, M.D. Professor of Radiology, New York University, College of Medicine, New York, N. Y.

Pasternack, Richard, Ph.D., Organic Chemistry, Antibiotics. Director of Research, Chas. Pfizer and Company, Inc., Brooklyn, N. Y.

Rabinowitz, Harold M., M.D., Physics and Chemistry as related to Pathologic Physiology. Senior Surgeon, Beth Moses Hospital; Consulting Surgeon, Jewish Sanatorium and Hospital of Chronic Diseases, Brooklyn, N. Y.

ACTIVE MEMBERSHIP

Abell, Richard G., Ph.D., Living Blood Vessels. Assistant Professor of Anatomy, Medical School, University of Pennsylvania, Haverford, Pa.

Albanese, Anthony A., Ph.D., Chemistry and Clinical Application. Assistant Professor, Department of Pediatrics, School of Medicine, New York University, New York, N. Y.

Algire, Glenn H., M.D., Cancer Research, Vascular Physiology. Senior Assistant Surgeon, U. S. Public Health Service, National Cancer Institute, Bethesda, Md.

Anderson, Tirzah W., B.A., M.A., Psychology, Biology. Consultant, Counseling and Faculty Life, Education, National Board, Y. W. C. A., New York, N. Y.

Ashby, Winifred, Ph.D., Immunology, Bacteriology. Senior Scientist, Federal Government, St. Elizabeth's Hospital, Washington, D. C.

Bass, Biren, B.S., Chemistry, Biology. Manager, By-Product Sales, Market Development New Products, Publicker Industries, Inc., Philadelphia, Pa.

Bassett, Samuel H., M.D., Internal Medicine, Human Metabolism. Associate Physician, Strong Memorial and Rochester Municipal Hospitals, Rochester, N. Y.

Batterman, Robert Coleman, M.D., Therapeutics, Medicine. Instructor in Therapeutics, College of Medicine, New York University, New York, N. Y.

Beatty, Clarissa H., Ph.D., Physiology. Instructor, College of Physicians and Surgeons, New York, N. Y.

Been, Robert B., B.S., Physics and Chemistry. Assistant Director, New York School of Optics, New York, N. Y.

Bernardi, Dominic J., Ph.D., Chemistry. Interchemical Corporation, New York, N. Y.

Binkley, Francis, Ph.D., Biochemistry, Clinical Chemistry. Assistant, Hospital of the Rockefeller Institute, New York, N. Y.

Black, Archie, Ph.D., Nutrition, Biochemistry. Head, Nutritional Products Development Laboratory, E. R. Squibb & Sons, New Brunswick, N. J.

Blanchard, Ernest W., Ph.D., Physiology, Biochemistry. Director of Research, Schieffelin and Company, New York, N. Y.

Bottorff, Charles A., D.V.M., Biology. Poultry Pathologist, Lederle Laboratories, Inc., Pearl River, N. Y.

Bourdillon, Jacques, M.D., Physical Chemistry of proteins and viruses. Senior Medical Biochemist, New York State Department of Health, Albany, N. Y.

Boxer, George E., Ph.D., Biological Chemistry. Senior Chemist, Biochemistry, Merck and Company, Rahway, N. J.

Braitberg, Leo D., M.S., Chemistry, Biology. Research Chemist, Pyridium Corporation, Yonkers, N. Y.

Brauer, Ralph W., Ph.D., Biological Chemistry. Instructor in Pharmacology, Harvard Medical School, Boston, Mass.

Brink, Frank, Jr., Ph.D., Physical Chemistry of Cells and Tissues. Associate in Biophysics, Johnson Research Foundation, University of Pennsylvania, Philadelphia, Pa.

Brooks, Chandler McC., Ph.D., Physiology. Associate Professor of Physiology, The Johns Hopkins University, School of Medicine, Baltimore, Md.

Bucca, Matthew A., Ph.D., Bacteriology. V. D. Research Laboratory, U. S. Marine Hospital, Staten Island, N. Y.

Buchbinder, Leon, Ph.D., Bacteriology, Immunology. Senior Bacteriologist, Department of Health, New York, N. Y.

Burch, George E., M.D., Biology. Associate Professor of Medicine, Tulane Medical School, Tulane University, New Orleans, La.

Burkholder, Paul E., Ph.D., Microbiology, Plant Physiology. Professor of Botany, Yale University, New Haven, Conn.

Bush, Frederic A., A.B., Geological Sciences. Chief Geologist, Sinclair Oil Corporation, New York, N. Y.

Butterfield, George P., M.A., Biology, Chemistry. Consultant, Bacteriology and Chemistry, Chicago, Ill.

Caldwell, A. Lee, Ph.G., Ph.C., Nutrition, Pharmaceutical and Medicinal Applications. Head, Vitamin Research Department, Lilly Research Laboratories, Indianapolis, Ind.

Calkin, John B., M.A., M.S., Chemical Engineering. Assistant to Vice President, Union Bag and Paper Corporation, New York, N. Y.

Carpenter, C. LeRoy, A.B., B.S., Chemistry, Chemical Engineering. Graduate Student, Columbia University, New York, N. Y.

Carpenter, Frederick H., Ph.D., Research Associate in Biochemistry, Cornell University Medical School, New York, N. Y.

Carrea, Raul Manuel Eduardo, M.D., Neurology. Assistant Resident in Neurosurgery, Neurological Institute of New York, New York, N. Y.

Chertkoff, Frieda, M.S., Bacteriology. Teacher, Junior High School, New York, N. Y.

Cochrane, Vincent W., Ph.D., Biology. Assistant Plant Pathologist, Connecticut Agricultural Experiment Station, New Haven, Conn.

Coghill, Robert D., Ph.D., Biochemistry, Organic Chemistry, Fermentation, Nutrition. Director of Research, Abbott Laboratories, North Chicago, Ill.

Commoner, Barry, Ph.D., Cellular Physiology, Science Writing. Medical Editor, "Science Illustrated," New York, N. Y.

Cowgill, George R., Ph.D., Nutrition, Biochemistry, Physiology. Professor of Nutrition, Yale Nutrition Laboratory, New Haven, Conn.

Curtis, Howard J., Ph.D., Biology, Physiology. Associate Professor of Physiology, Columbia University, New York, N. Y.

Daniels, George E., M.D., Psychiatry, Psychoanalysis, Psychosomatic Medicine. Clinical Professor of Psychiatry, Columbia University, New York, N. Y.

Daughenbaugh, Paul J., Ph.D., Biochemical and Pharmaceutical Research. Head, Pharmaceutical Research and Sales Service Laboratory, Merck and Company, Inc., Rahway, N. J.

Dawson, Charles R., Ph.D., Organic Chemistry, Biochemistry. Associate Professor, Chemistry, Columbia University, New York, N. Y.

Denstedt, Orville F., Ph.D., Biochemistry, Blood. Assistant Professor of Biochemistry, McGill University, Montreal, Canada.

De Simone, John, B.S., Biochemistry. William R. Warner, New York, N. Y.

Dexter, Lewis, M.D., Internal Medicine, Cardiology. Associate in Medicine, Harvard Medical School; Senior Associate in Medicine, Peter Bent Brigham Hospital, Boston, Mass.

Deyrup, Ingrith J., Ph.D. Instructor in Physiology, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Dibblee, Diana, M.S. Bacteriologist, Onyx Oil and Chemical Company, Jersey City, N. J.

Dickey, Lloyd B., M.D., Zoology, Tuberculosis, Pediatrics. Associate Professor of Pediatrics; Chief of Children's Clinic, San Francisco, Calif.

Dische, Zacharias, M.D. Research Worker, Biochemistry Department, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Dreyer, Nicholas B., M.A. (Oxon.), Gastroenterology. Professor of Pharmacology, University of Vermont, Burlington, Vt.

Du Bois, Adrian S., M.S., Chemistry, Bacteriology. Richards Chemical Works Division, Onyx Oil and Chemical Company, Jersey City, N. J.

Dunihue, Fred W., Ph.D., Histology of the Kidney, Hypertension. Associate Professor of Microscopic Anatomy, University of Vermont, College of Medicine, Burlington, Vt.

Duschinsky, Robert, Ph.D., Organic and Biological Chemistry. Senior Research Chemist, Hoffman-LaRoche, Inc., Nutley, N. J.

Dyer, Agnes S., B.S., Physical Chemistry, Surface Chemistry. Graduate Assistant, Parke-Davis Fellowship, Michigan State College, East Lansing, Mich.

Earle, Wilton R., Ph.D., Cancer Research. Principal Cytologist, National Cancer Institute, Bethesda, Md.

Eggerth, Arnold H., Ph.D., Bacteriology, Immunology. Associate Professor in Bacteriology, Long Island College of Medicine, New York, N. Y.

Ehrich, William E., M.D., Biology, Chemistry. Professor of Pathology, University of Pennsylvania, Philadelphia, Pa.

Ehrlich, Joseph C., M.D., Pathology. Director of Laboratories, Lebanon Hospital, Bronx, N. Y.

Eidelberg, Ludwig, M.D., Psychoanalysis. Chief Psychologist, Mount Sinai Hospital, New York, N. Y.

Eiger, Irena Z., M.Sc., Enzyme Chemistry. Assistant, Department of Chemistry, Columbia University, New York, N. Y.

Elias, William F., Ph.D., Antibiotics, Bacteriology. Technical Director, Antibiotics Division, Wyeth, Inc., Philadelphia, Pa.

Elmadjian, Fred, M.S. Physiologist, Memorial Foundation of Neuro-Endocrine Research, Worcester State Hospital, Worcester, Mass.

Elman, Robert, M.S., M.D., Biochemical Research in Surgical Diseases. Associate Professor of Clinical Surgery, Barnes Hospital, St. Louis, Mo.

Enders, Robert K., Ph.D., Biology. Professor of Zoology, Swarthmore College, Swarthmore, Pa.

Erlich, Victor L., Ch.E., Ch.D., Protein and Enzyme Chemistry, Technology. New York, N. Y.

Farber, Sidney, M.D., Pathology, Pathologic Physiology. Assistant Professor of Pathology, Harvard Medical School, Boston, Mass.

Firth, Francis G., M.Sc., Structure Analysis. Research Engineer, North American Philips Corporation, New York, N. Y.

Fischer, Earl K., Ph.D., Physical Chemistry. Head, Physical Chemistry Division, Institute of Textile Technology, Charlottesville, Va.

Fischer, Hermann O. L., Ph.D. Research Professor of Organic Chemistry, University of Toronto, Toronto, Canada.

Fisher, Saul H., M.D., Internal Medicine, Physiology. Instructor of Medicine, New York University College of Medicine, New York, N. Y.

Frant, Samuel, M.D., Biology. Deputy Commissioner, Department of Health, New York, N. Y.

Fraps, Richard M., Ph.D., Research, Physiology. Senior Physiologist, U. S. Department of Agriculture, Beltsville, Md.

Free, Alfred H., Ph.D., Biochemistry, Nutrition. Assistant Professor of Biochemistry, Western Reserve University, Cleveland, Ohio.

Freund, Jules, M.D., Immunology, Infectious Diseases, Virus, Bacteriology, Pathology. Assistant Director, Bureau of Laboratories, Department of Health, New York, N. Y.

Frey, Charles N., Ph.D., Chemistry. Director of Scientific Relations, The Fleischmann Laboratories, New York, N. Y.

Fritzsching, Erwin T., Biochemical Research, Production of Nutritional and Medicinal Compounds. Divisional President, Biochemical Division, Interchemical Corporation, Union, N. J.

Frost, Douglas V., Ph.D., Nutrition, Biochemistry. Head of Nutrition Research, Abbott Laboratories, North Chicago, Ill.

Fulmer, Ellis I., Ph.D., Physiology, Biochemistry. Professor of Chemistry, Iowa State College, Ames, Iowa.

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NOVEMBER 4, 1946

DOCTOR E. S. MOORE, University of Toronto, Toronto, Canada: *Origin of Iron Deposits of the "Lake Superior" Type.*[*] (This lecture was illustrated by lantern slides.)

INTRODUCTION

The iron deposits which form the subject of this article constitute a peculiar type, presenting in their nature and origin some of the most puzzling problems in geology. They are peculiarly restricted, as far as is known, to the pre-Cambrian sequence, but they are widely distributed geographically, being found on all continents. The deposits on the various continents exhibit certain common features which make it impossible to distinguish between specimens obtained from these widely scattered areas.

Why should such deposits be confined to the pre-Cambrian and not reappear in later sequences? In the earlier days of geology, there was a tendency to invoke special physico-chemical and meteorological conditions in the pre-Cambrian era that have not existed since that time. To-day, however, we tend more and more to assume that geological processes and conditions have followed a series of rules well controlled by physical and chemical laws, through all the geological periods. It is rare, nowadays, that a geologist will seek to explain peculiar phe-

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nomena on any other basis than these well-known laws. It may be necessary to assume peculiar combinations of circumstances and activities which produce conditions, every factor in which is not evident because of some missing item necessary for complete knowledge of the situation.

We have other great problems in geology that await solution. The causes of continental glaciation, and the accumulation of great quantities of petroleum in certain periods are two problems not yet fully solved. We believe that the solution will be found in the application of knowledge not beyond that of the modern scientist. It will be found in a special combination of conditions or activities, each one of which in itself may be fairly simple. The complexity of the combination appearing in one era may in some cases be so great that it does not reappear in other eras. It is thus, I believe, with these puzzling pre-Cambrian iron formations. The more we study the great complex known as the pre-Cambrian sequence, the more we are convinced that the geological processes of igneous activity, mountain building, erosion, and sedimentation during this earliest era were governed by the same laws as they were in the later eras. The main differences between the pre-Cambrian era and the later ones were in the relative amount of igneous activity and life, and the nature of the life which participated in geological processes.

Banded Iron Formation

The iron deposits which we are discussing are peculiar in being closely associated with a highly siliceous, well-banded formation, commonly known as "banded iron formation." The banding and the high silica content are the two prominent features, common to at least the greater part of all the formations, no matter where found. The silica may run from about 2 or 3 per cent in high grade ores found in some of the banded formations to about 99 per cent in the very siliceous types. The rock high in silica has more nearly perfect banding than that low in this substance. There may be practically nothing but magnetite or hematite with the silica, especially in the older systems like the Keewatin and Timiskaming. The iron oxides are in some cases fairly uniformly distributed through the silica, as they are in jasper, but in most of the formations they tend to be concentrated along darker bands.

There are, in some formations, in addition to hematite or magnetite or both, other iron-bearing minerals, such as greenalite, a hydrous ferrous-magnesian silicate of green color, or chamosite, a similar member of the chlorite group, and siderite, hornblende, actinolite, and grünerite.

My experience has been that greenalite or chamosite are rare in the older iron formations, but abundant in the later ones such as those of the later Huronian, and the Animikie. The same is true of siderite, and I have found that, in a great many cases at least, this mineral has been introduced into the banded iron formations by hot waters.

Further, the silica in the older formations is cryptocrystalline to finely crystalline, even quite coarsely granular in some places, and not concretionary, as it is in many of the Proterozoic formations. The greenalite is also almost invariably found in öolites, and the hematite assumes öolitic form, due partly to derivation of some of it from öolitic greenalite and partly to direct precipitation in concretionary form.

The banding is most perfect in the older formations. This can be well observed in some places in the Lake Superior region where Keewatin and Animikie iron formations occur in the same locality, as on the Matawin Range. The Biwabik formation of the Mesabi Range is less well banded than the Soudan formation in the Vermilion Range, or scores of small Keewatin ranges in other parts of the Canadian Shield. The öolitic structure in the Biwabik formation is also well developed. I think that metamorphism had something to do with bringing out the banding, and where the iron content is high compared with the silica, the banding is less well developed. Where iron oxides are practically absent, the silica is often found in thicker bands.

The post-Cambrian sedimentary iron formations, such as the Clinton and Wabana of the Palaeozoic, and the Mesozoic minettes of France, are highly öolitic and lack the distinctive banding of the pre-Cambrian types. It would appear that the later pre-Cambrian formations were the precursors of those of the post-Cambrian. In view of the great role played by life in the formation of öolites of carbonate, one might well raise the question as to whether some form of life, such as iron bacteria or algae, for example, may not have begun in the late pre-Cambrian to play an important role in the deposition of the iron formations.[1]

Some of the banded iron formations contain no iron ore whatever. Others can be mined as ore which, it is believed, was laid down at the time the silica-rich deposit was formed; that is, it is primary in character. Such ore is found in the Goulais River Range in Ontario, and in a number of other deposits. The material requires magnetic concentration, because the iron only runs, on the average, about 32 to 33 per cent. The remainder is almost entirely silica and, fortunately, the iron is almost all in magnetite. Some of the formation runs higher and some lower than these figures, but one of the striking features of the greater part of the formations is the remarkable uniformity in the grade of the ore. There is no evidence that there has been any appreciable concentration of iron by supergene or hydrothermal action since the formation was originally deposited.

In the Lake Superior region of the United States, on the other hand, and in those rich hematite deposits now being developed in eastern Quebec and in western Labrador, there is much evidence of supergene concentration of the ore. There are places in some of the iron formations where hematite appears to have been deposited in sufficient proportions to make primary low grade ore. I have seen such lenses on the Belcher Islands, Hudson Bay, and elsewhere, but all large ore bodies show evidence of extensive concentration of iron in the banded formations. Personally, I believe that these concentrations occurred mainly in pre-Cambrian time, at or near the surface where the formations were exposed to weathering before being buried by later formations, folded, and metamorphosed. Some of the ore bodies now lie at great depth. Gruner[2] has advocated concentration by hydrothermal action in the iron formations. The evidence from the Mesabi Range is particularly strong in favor of supergene concentration during pre-Cambrian and later periods.

There are large bodies of iron carbonate in the Michipicoten area of Ontario. They, too, are all associated with banded iron formation, and they present special problems of their own. They will be discussed later.

An impressive feature of the banded formations is the exceedingly intricate folding so often seen in them. Data for whole chapters on structures can be found in many small areas. The manner in which these hard brittle rocks which, in a given area, were complexly folded at one time and highly brecciated at another, raises questions regarding the physical condition of the material. I have come to the conclusion that the silica and iron were laid down in a colloidal state, and that before they were crystallized to a hard, brittle state they folded very readily. Later, after becoming cryptocrystalline and brittle, they brecciated more readily than they folded.

ORIGIN OF THE BANDED FORMATION

It is very difficult to find a completely satisfactory explanation for the origin of these peculiar rocks and to elucidate why they are confined to the pre-Cambrian. A number of theories have been suggested, but none of them fully satisfies all requirements. The theories may be divided into two main classes, those based on igneous origin and those which consider the formations to be sedimentary. As in so many cases in geology, a combination of parts of two or more theories may help to explain exceptional conditions.

Van Hise and Leith offered a hypothesis based on igneous activity.[3] According to them, hot lava flows pouring into sea water set up a reaction between the lava and the water, producing sodium silicate of the nature of water glass, and iron chloride. The waters, on cooling, precipitated iron, silica, and hydrous iron silicate of the greenalite type. Laboratory experiments showed that such reactions could be demonstrated. When applied to field conditions, however, there are serious difficulties, because this explanation made necessary a repetition of many lava flows to build up a banded formation of considerable thickness, and such flows are rare or entirely absent in many of the iron ranges.

J. W. Gruner has, in several papers, supported the theory of weathering of land surfaces followed by deposition of iron and silica as chemical sediments. He puts up a very strong case for this idea. I have always strongly favored it, with some modification for some very siliceous formations in the Keewatin system. It seems probable that in highly volcanic areas considerable silica may have been supplied by hot springs and by waters heated by lava flows entering basins of water.

Lucien Cayeux, of Paris, has advocated the leaching of peneplained lands as the source of the iron in the French sedimentary iron deposits.

Dunn,[4] of India, has recently stated that he believes that some of the banded hematite in that country is of replacement origin. T. L. Tanton, of the Canada Geological Survey, has attempted to prove that the banded material was injected in hot solutions. There is, however, little evidence to support this notion, as no case was found of cross-cutting relations between the banded silica and enclosing formations, such as would be expected to occur if deposition had been from hot solutions.

It is quite possible to have rocks as well banded as these by replacement of thinly bedded sediments. The fluorite deposits in Illinois and the lead and zinc ores in the Sullivan Mine in British Columbia are some examples. However, in Illinois, the country rock is a thin-bedded argillaceous limestone, and at the Sullivan Mine it is a thin-bedded argillite and the bands of ore reflect perfectly the original banding. No such thin-bedded rocks occur in most of the iron ranges to be replaced by the silica and iron and thus produce a pseudomorphous banded formation.

In recent years, I have been doing a lot of field work on these iron deposits and have collected some new data bearing on their origin. For example, in the Michipicoten district of Ontario, there are a number of siderite deposits closely associated with extensive formations of banded silica. The silica member in every range rests upon acid formations, mainly tuff and agglomerate with some rhyolite. The silica contains, in most places, practically no iron except some carbonate and sulphide which were unquestionably introduced long after the silica was deposited. In many other ranges, where the banded formation lies on basic rock, there is always much more iron with the silica member. I think this is strong evidence that the iron and silica were leached from the country rocks in the vicinity of bodies of water and deposited in them as chemical sediments.

I have always been a strong believer in the study of geochemical problems in the laboratory. This represents an immense field for investigation where one can imitate natural processes. It seemed to me that too little was known about the behavior of iron and silica under natural conditions, and I encouraged a graduate student, J. E. Maynard, now at Syracuse University, to work in the laboratory on these elements. He performed a great many careful experiments and we combined our field and laboratory data into a long paper.[5] Many interesting results bearing on the problems of the iron formations were obtained.

A number of rather surprising things were found. It was shown that iron, unlike calcium, is not carried as bicarbonate in natural surface solutions, but mainly as a ferric oxide hydrosol. The silica, up to 30 parts per million, is carried mainly in the colloidal state. Dr.

T. S. Lovering has recently expressed the opinion that some of the silica is ionized, and this seems quite possible. Colloidal silica and colloidal iron mutually precipitate one another, unless stabilized by an organic colloid. The influence of the stabilizing colloid loses its effect when electrolytes come in contact with these colloids.

It was found, further, that when the iron and silica entered water of the composition of sea water, the iron precipitates quite rapidly and the silica more slowly. Thus, a banded deposit may be built up by supplying colloidal solutions to a basin of salt water. It seems probable that these basins were sheltered bays on the oceans, inland seas, or salt lakes. Structures somewhat resembling the concretionary structures in some of the iron ranges were also developed.

From field and laboratory investigations, I strongly favor the sedimentary origin for most of the iron and silica. Laboratory experiments show that solutions of organic matter in which it is believed that CO_2 is the main active agent, or other solutions containing CO_2, are capable of producing sufficient colloidal iron and silica to make most of the banded iron formations. Further, there is enough iron and silica being carried by some streams today to make appreciable deposits. It seems probable that this large amount of finely comminuted rock, occurring as tuff in many of the iron regions, helped to supply much of the material. Also, the fresh vesicular lavas which break down readily under weathering conditions would supply the necessary substances. Woolnough[6] has recently stated that in Australia, where the duracrust is forming, conditions suitable for the collection of large quantities of iron and silica in salt water basins are found.

There remains the problem of how the organic colloid from peat solution to stabilize the colloids of iron and silica was supplied, way back in the pre-Cambrian. Personally, I believe that sufficient aquatic plants existed even in the early pre-Cambrian to do this. It takes only 9 parts per million of organic matter to stabilize 10 p.p.m. of ferric oxide hydrosol and 30 p.p.m. of colloidal silica, the average proportions which Gruner suggests exist in the iron ranges.

THE IRON CARBONATE DEPOSITS

The siderite deposits of the Michipicoten district, Ontario, differ from all the others described, in that the siderite and associated pyrite are found with banded silica, while the ore was emplaced in a much later period than the banded silica. Further, the silica is almost free from iron oxide in most places. It invariably lies on acid pyroclastics or flows.

Collins[7] assumed that the silica was deposited by hot springs and the siderite and pyrite beneath it, through replacement by rising thermal waters. Grout[8] very early pointed out the difficulty involved in this conception. Later, I observed some spacial relations among certain igneous intrusions, gold and sulphide deposits, and extensive carbonatization, which suggested that the siderite deposits were related to the same magmas as these other features. Hawley[9] showed that the pyrite and siderite had been introduced at a much later period than the silica. Recent mapping of the ranges in detail, by myself and Dr. H. S. Armstrong, for the Ontario Department of Mines, has shown that the siderite and associated pyrite were introduced after the banded silica was folded and in some places highly brecciated. The banded silica member of the iron formation always occurs in the hanging wall of the ore bodies. It exerted structural control in the emplacement of the siderite and pyrite which replaced, mainly, acid pyroclastics and some lavas. These minerals show a great preference for acid rocks in replacing the formations. Basic lavas stratigraphically above the banded silica are practically unaffected by the replacing solutions.

Why should so much carbonate ore be formed with the banded silica in Michipicoten, and so little in the other iron ranges? There is a peculiar situation here, in that there is an immense amount of ottrelite in the acidic rocks beneath the iron formation in that region. I have never seen anything comparable elsewhere in the Canadian Shield. There are also a great many dikes near the ore bodies, and some of them are full of ottrelite. Many dikes belong to the diorite family, some are kersantites, or mica diorites, and there are lamprophyres and other rocks of the intermediate type. There is granite in the offing, and it is believed that the ore bodies and dikes are of Algoman age and that they all came from a common magma.

Another strange feature is that at the Josephine Mine the ore is good red hematite, although it is in exactly the same position as the siderite in all the other ore bodies. It lies along the footwall of the banded silica, with carbonate to the east and west of it. The hematite is undoubtedly of hydrothermal origin and shows no definite evidence of having been formed by replacement of carbonate or alteration of it.

The explanation suggested to explain the presence of the hematite in this carbonate zone is that the hematite and carbonate came from a common source; that considerable strike faulting occurred along the footwall of the banded silica member where the hematite is found; and that this was a factor in forming the hematite. There is, much later, transverse faulting in the carbonate deposits, but no evidence of strike faulting.

Around 500° C., the stability relations between siderite and hematite may be affected by changes in pressure. It is believed that with faulting occurring at the time when the ores were being deposited, there was a release of pressure in the zone where the hematite is found, and that this mineral was therefore deposited instead of siderite, with the minerals coming from the same magmatic source.

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SECTION OF BIOLOGY

NOVEMBER 11, 1946

DOCTOR GEORGE H. CHAPMAN, Clinical Research Laboratory, New York, N. Y.: *The Staphylococci.** (This lecture was illustrated by lantern slides.)

The literature of staphylococci abounds with widely conflicting, vague and empirical statements. Technical methods, whose tremendous variations play a major role in the discrepancies, too often are changed at the whim of the investigator and are inadequately described. An attempt is made to analyze the basic principles involved in the isolation and testing of these organisms and to develop more reliable methods.

The taxonomy is in a chaotic state, because too much emphasis is placed on unstandardized biochemical and physiological properties which show marked differences with minor changes in technique. Investigators have proposed differentiation of species into "*aureus*," "*albus*," "*pyogenes*," "*saprophyticus*," etc., and even a trinomial, "*Staphylococcus aureus hemolyticus*," without regard to the fact that dissociation, which is extremely common in this genus, results, *e.g.*, in the presence of white variants in orange cultures and non-hemolytic variants in hemolytic cultures, often in the first subculture.

Staphylococci associated with inflammatory processes assume a number of biochemical, physiological, and pathogenic properties which are lacking in cultures from other sources or in old stock cultures. Hence, the differentiation should be into pathogenic and non-pathogenic strains. This association is far from absolute, because (1) lowered resistance of the host permits feeble parasites or even saprophytes to invade the tissues; (2) there are considerable differences in severity of the inflammatory lesions, usually with parallel differences in the properties of the cultures; and (3) the genus is so prevalent that it is a common contaminant: not only are pathogenic types common in non-pathogenic habitats but non-pathogenic types occur in pathogenic habitats. Because there is no precise definition of "pathogenicity," it is better to refer to an organism as the pathogenic, non-pathogenic, or intermediate "type."

Pathogenic types rapidly lose some of their properties on cultivation or storage, resulting in degenerate intermediates which may have positive, weakly positive, or negative reactions to any of the tests and, because not all the cells of a culture degenerate at the same rate or in the same manner, the cultures usually are kaleidoscopic mixtures of variants, each having different properties. The most degenerate types (saprophytes and G forms) usually react negatively to these properties. Certain reactions, *e.g.*, coagulase and α-hemolysin, are given by almost all pathogenic types but by extremely few non-pathogenic types and are used as *in vitro* indicators of pathogenicity. Other reactions are given by fewer pathogenic types and by more non-pathogenic types. They are unsuitable for this purpose.

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* Aided by grants from the Ophthalmological Foundation.

Most pathogenic types have smaller cells (about 0.6μ, compared with 1.0μ in the non-pathogenic group); they produce orange growths which are hemolytic for rabbit, and sometimes other, erythrocytes; they are consistently gram-positive, whereas many saprophytes and G forms are gram-variable; they have strong reducing power; they are gelatinolytic, lipolytic, and fibrinolytic; they produce substances which clot plasma, are toxic for the skin, gastrointestinal tract, and conjunctivae, and kill laboratory animals; they produce a polysaccharide, agglutinins, and precipitins which are different from those of the non-pathogenic type; they produce more alkali in carbohydrate-free media than do the saprophytes; they produce a factor which increases tissue permeability; they resist the antibacterial power of fresh blood and lysozyme; they are more tolerant to sodium chloride; they clot milk; and they ferment dextrose, levulose, saccharose, glycerol, maltose, lactose, galactose, mannitol, d-mannose, and trehalose—but not dextrin, cellobiose, salicin, inulin, inositol, dulcitol, arabinose, xylose, raffinose, rhamnose, starch, mellibiose, adonitol, sorbitol, and melezitose. Non-pathogenic types have fewer of these properties.

Usual methods for isolation are hopelessly inadequate, particularly when other bacteria are present. "Blood agar" plates frequently are overgrown by other bacteria, and the hemolysis of horse and human blood so often used in its preparation has little significance. "Nutrient agar" is not selective and gives poor development of pigment. Attempts to find a selective isolation medium led to the development of alkaline bromthymol blue lactose agar, phenol red mannitol agar (Difco), alkaline bromthymol blue lactose agar with potassium tellurite and, finally, phenol red mannitol agar with 7.5 per cent sodium chloride. Because several writers complained of the amount of work involved in isolating and testing staphylococci by the methods proposed, experiments were undertaken to try to develop a culture medium which would permit isolation on the basis of chromogenesis and at the same time provide easier application of coagulase and mannitol fermentation tests. Study of a number of ingredients showed that, for enhancing chromogenesis, mannitol was far superior to lactose but that a small amount of lactose gave more luxuriant growth and did not interfere with tests for fermentation of mannitol; Bacto yeast extract was decidedly better than meat extract and, with lactose, it overcame the inhibition of non-pathogenic types by sodium chloride; many peptones were unsuitable, Bacto tryptone (but not tryptose) being among the best; sodium chloride provided excellent inhibition of other bacteria and enhanced chromogenesis; and gelatin further improved chromogenesis and permitted direct application of the Stone reaction. Each ingredient was tested in several concentrations, and the optimum amount was selected. Conditions which improved chromogenesis also were more favorable for the coagulation of plasma.

The medium finally accepted (staphylococcus medium No. 110) made possible the selective isolation of staphylococci even when outnumbered 10 millions to 1 by other bacteria. It gave good pigment production, and the coagulase, mannitol fermentation, and Stone reactions could be applied directly to the original plates. It had the following composition: Water, 1,000 ml; agar, 15 grams; Bacto yeast extract, 2.5 grams; Bacto tryptone, 10 grams; mannitol, 10 grams; lactose, 2 grams; K_2HPO_4, Merck reagent, 5 grams; sodium chloride, 75 grams; gelatin, 30 grams. The following procedure is indicated: Adjust to pH 7.0 and sterilize in the autoclave. Incubate 42 to 43 hours for coagulase and 48 hours for the other tests. At 48 hours incubation, observe the color of the growth and select colonies for transplants. Place one drop of 0.04 per cent bromthymol blue on the cleared area left from the coagulase test. A yellow or greenish-yellow color indicates fermentation of mannitol. Add 10 ml. of ammonium sulfate solution that has been kept in the incubator with an excess of crystals (*i.e.*, supersaturated) and observe any clearing around the discrete pigmented colonies, which begins in about 5 minutes and is complete in about 30 minutes. Estimate the width of the clear zones as negative, weakly positive (about 1 mm.), and positive (2 to 4 mm.).

For the coagulase test, either apply the slide technique, which consists of emulsifying a colony in a drop of saline, adding a drop of plasma, and observing almost immediate clumping, or apply the tube technique as follows: Incubate test strain No. 64 on staphylococcus medium No. 110 for 42 to 43 hours. For each blood sample, gather as much growth as possible into a 4 mm. loop and emulsify it in 0.1 ml. of Bacto brain heart infusion, which accelerates clotting, then let it stand exactly 15 minutes with frequent vigorous shaking. Add 0.2 ml. of the blood to be tested. Incubate and examine after 20 minutes. Select the samples that show a distinct but not necessarily firm clot. Most rabbit, but few human, samples are satisfactory. For the test, proceed similarly but use the culture to be tested and the selected blood, read the results in one hour and re-examine negatives in 5 hours, considering any definite evidence of clotting as positive.

These methods have proved to be valuable in the study of human and bovine infections, and in staphylococcus food poisoning. In the latter instances, staphylococci were readily found by us in large numbers, whereas another laboratory found none. By testing the nose and throat of food handlers and the feces of suspected victims, and comparing the reactions with those of staphylococci isolated from the peccant food, it is possible to trace the origin of the outbreaks, to establish that the victim has staphylococcus food poisoning and not another type of gastroenteritis, and to prevent outbreaks by eliminating carriers of food-poisoning staphylococci. Application of the Stone reaction by this direct method has verified the claim of Stone that the reaction is positive when applied to freshly isolated (pigmented) strains. In the series of outbreaks studied here, the reaction was positive in all incriminated staphylococci, and the strains from suspected handlers had identical properties and width of Stone zoning as cultures from the peccant food.

SECTION OF PSYCHOLOGY

NOVEMBER 18, 1946

DOCTOR J. MCVICKER HUNT, The Institute of Welfare Research, Community Service Society, New York, N. Y.: *Measuring Effects of Case Work on the Client.* (This lecture was illustrated by lantern slides.)

(An abstract of this paper will be published
in the January issue of TRANSACTIONS.)

SECTION OF ANTHROPOLOGY

NOVEMBER 25, 1946

DOCTOR KARL A. WITTFOGEL, Columbia University, New York, N. Y.: *The Significance of Large-Scale Waterworks in the Higher Civilizations of Pre-Conquest America.*

(No abstract received.)

SECTION OF BIOLOGY

November 8, 1946

Conference on *"Bioluminescence."*

The Section of Biology held a Conference on "Bioluminescence." Doctor E. Newton Harvey, Princeton University, Princeton, N. J., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"A General Survey of Bioluminescence," by E. Newton Harvey, Princeton University, Princeton, N. J.

"Chemiluminescence in Aqueous Solution," by Rubert S. Anderson, University of Maryland Medical School, Baltimore, Maryland.

"Chemistry of *Cypridina* Luciferin," by Aurin M. Chase, Princeton University, Princeton, N. J.

"The Fundamental Action of Pressure Temperature and Drugs on Enzymes, as Revealed by Bacterial Luminescence," by Frank H. Johnson, Princeton University, Princeton, N. J., and Henry Eyring, University of Utah, Salt Lake City, Utah.

"The Microscopic Anatomy and Physiology of Light Production in the Firefly," by John B. Buck, National Institute of Health, Bethesda, Maryland.

NOVEMBER 15 AND 16, 1946

Conference on *"The Biology of Normal and Atypical Pigment Cell Growth."*

The Section of Biology held a Conference on "The Biology of Normal and Atypical Pigment Cell Growth." Doctor Myron Gordon, New York Zoological Society, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

FRIDAY, NOVEMBER 15

Morning Session. Chairman, R. G. Harrison, Yale University, New Haven, Connecticut.

"The Melanoma Problem," by George T. Pack, Memorial Hospital, New York, N. Y.

"The Origin and Development of the Melanophores," by G. P. DuShane, Stanford University, Palo Alto, California.

"Pigment Cells in Man," by P. Masson, University of Montreal, Montreal, Canada.

"Dermatological Investigations of Melanin Pigmentation," by S. William Becker, University of Chicago School of Medicine, Chicago, Illinois.

"Relationship of Pigment Cell Clusters to Ocular Melanoma," by Helenor Campbell Wilder, The American Registry of Pathology, Army Medical Museum, Washington, D. C.

Afternoon Session. Chairman, W. E. Heston, National Cancer Institute, Bethesda, Maryland.

"Genetic Aspects of Pigment Cell Growth in Man," by Madge Macklin, University of Ohio, Columbus, Ohio.

"Growth and Vascular Reactions of Transplanted Mouse Melanomas," by Glenn H. Algire, National Cancer Institute, Bethesda, Maryland.

"Pigment Cell Tumors in Reptiles," by H. G. Schlumberger and Balduin Lucké, University of Pennsylvania School of Medicine, Philadelphia, Pennsylvania.

"The Cytology of the Typical and the Amelanotic Melanoma," by Michael Levine, Montefiore Hospital, New York, N. Y.

"Genetic Aspects of Normal and Atypical Pigment Cell Growth in Fishes," by Myron Gordon, New York Zoological Society, New York, N. Y.

SATURDAY, NOVEMBER 16

Morning Session. Chairman, V. C. Twitty, Stanford University, Palo Alto, California.

"Hormonal Influences on Pigmentation in Birds," by B. H. Willier, Johns Hopkins University, Baltimore, Maryland.

"Influence of the Endocrine Status upon Pigmentation in Man and in Mammals," by James B. Hamilton, Long Island College of Medicine, Brooklyn, N. Y.

"The Induction of Pigmented Tumors in Mice by Methylcholanthrene," by Leonell C. Strong, Yale University School of Medicine, New Haven, Connecticut.

"Effect of Various Factors on Transplantable Mouse Melanoma," by Kanematsu Sugiura, Memorial Hospital, New York, N. Y.

"Light and Melanin Pigment," by H. F. Blum, Guggenheim Fellow, Marine Biological Laboratory, Woods Hole, Massachusetts.

Afternoon Session. Chairman, Jesse P. Greenstein, National Cancer Institute, Bethesda, Maryland.

"Morphological Color Changes in Vertebrates," by Joseph M. Ordiorne, Georgetown University School of Medicine, Washington, D. C.

"A Classification of Melanins," by Howard S. Mason, National Institute of Health, Bethesda, Maryland.

"Some Features of Melanoid Biochromy in Animals," by Denis L. Fox, Scripps Institution of Oceanography, University of California, La Jolla, California.

"The Chemical Nature of Melanin," by F. H. J. Figge, University of Maryland School of Medicine, Baltimore, Maryland.

"Some Cytochemical Aspects of Pigment Cells," by M. J. Kopac, Washington Square College, New York University, New York, N. Y.

"Chemistry of Melanomas," by Jesse P. Greenstein, National Cancer Institute, Bethesda, Maryland.

"Physiological Genetics of Pigmentation in Mammals," by W. C. Russell, Elizabeth S. Russell, and Liane R. Braush, Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine.

SECTION OF PHYSICS AND CHEMISTRY

November 29 and 30, 1946

Conference on *"Chromatography."*

The Section of Physics and Chemistry held a Conference on "Chromatography." Doctor Harold G. Cassidy, Yale University, New Haven, Connecticut, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, November 29

"Introductory Remarks," by the Conference Chairman.

"The History, Scope, and Methods of Chromatography," by L. Zechmeister, California Institute of Technology, Pasadena, California.

"The Theory of Chromatography," by Henry C. Thomas, Yale University, New Haven, Connecticut.

"Chromatography by Optical Control of the Percolate," by Stig Claesson, Institute of Physical Chemistry, University of Uppsala, Sweden.

"Some Experiments in Systematic Quantitative Chromatography," by W. A. Schroeder, California Institute of Technology, Pasadena, California.

Saturday, November 30

"Fractionation and Analysis of Hydrocarbons by Adsorption," by Beveridge J. Mair, National Bureau of Standards, Washington, D. C.

"Stereochemistry and Chromatography," by L. Zechmeister, California Institute of Technology, Pasadena, California.

"Chromatography in the Streptomycin Problem," by Robert L. Peck, Merck and Company, Inc., Rahway, N. J.

"Partition Chromatography," by A. J. P. Martin, Messrs. Boots, Pure Drug Company, Ltd., Nottingham, England.

"Partition Chromatography of Amino Acids and Related Compounds," by Stanford Moore and William H. Stein, The Rockefeller Institute for Medical Research, New York, N. Y.

"Chromatography in the Isolation and Separation of the Various Penicillins," by Jacques L. Wachtel, Schenley Laboratories, Inc., Lawrenceburg, Indiana.

"Closing Remarks," by the Conference Chairman.

NEW MEMBERS

ELECTED NOVEMBER 21, 1946

SUSTAINING MEMBERSHIP

Daly, Sylvester, M.D., Bacteriology, Biochemistry. Instructor in Otolaryngology, College of Physicians and Surgeons, Columbia University; Attending Otolaryngologist, Presbyterian Hospital and Babies' Hospital, Columbia University, New York, N. Y.

Winsten, Walter Abbott, Ph.D., Biochemistry. Acting Laboratory Director, Antibiotics, Schenley Distillers Corporation, Larchmont, N. Y.

ACTIVE MEMBERSHIP

Abajian, John, Jr., M.D., Physiology. Associate Professor of Surgery (Anaesthesiology), University of Vermont College of Medicine, Burlington, Vt.

Bell, William Brown, M.A. President, American Cyanamid Company, New York, N. Y.

Bikerman, Jacob Joseph, Physical Chemistry. Merck and Company, Inc., Rahway, N. J.

Bonner, David, Ph.D., Microbiology, Genetics. Research Associate in Botany, Yale University, New Haven, Conn.

Bretnall, Pearl, Ph.D., Child Psychology. Assistant Professor, Department of Psychology, Brooklyn College, Brooklyn, N. Y.

Brown, M. Vertner, Ph.D., Physics and Biophysics. Instructor in Physics, College of the City of New York, New York, N. Y.

Clark, Mary C., B.S., Pharmacology, Bacteriology. Research Associate, Department of Pharmacology, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

Collias, Nicholas E., Ph.D., Social Life of Animals, Ornithology. Instructor in Biology, Department of Biology, Amherst College, Amherst, Mass.

Cosgrove, Rachel, B.S. Research Associate in Pharmacology, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

de Suto-Nagy, G. I., Ph.D., Chemistry, Biology. Research Fellow, Sterling Chemistry Laboratory, Yale University, New Haven, Conn.

Eder, Howard Abram, M.D., Internal Medicine, Biochemistry. Assistant, and Assistant Resident Physician, Hospital of the Rockefeller Institute, New York, N. Y.

Freedman, Alfred M., M.D., Pathology, Physiology. Assistant, Department of Pathology, Mount Sinai Hospital, New York, N. Y.

Gabrilove, Jacques L., M.D., Biochemistry, Clinical Investigation. Blumenthal Fellow, The Mount Sinai Hospital, New York, N. Y.

Goldring, William, M.D. Associate Professor of Medicine, New York University, Medical College, New York, N. Y.

Gorbman, Aubrey, Ph.D., Biology, Endocrinology. Assistant Professor, Zoology, Barnard College, New York, N. Y.

Groupe, Vincent, Ph.D., Microbiology. Senior Research Associate, Squibb Institute, Princeton, N. J.

Grumbach, Leonard, Ph.D., Physiology, Pharmacology. Instructor in Pharmacology, College of Dentistry, New York University, New York, N. Y.

Healy, Adelaide, A.B., Anthropology, Psychology. President, American Association of University Women, Garden City Branch, New York, N. Y.

Hine, Carolyn Hall, B.S., Pharmacology. Pharmacology Research Assistant, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

Hope, Fred J., B.S., Organic Chemistry. Research Chemist, Brooklyn Polytechnic Institute, Brooklyn, N. Y.

Johnstone, Donald B., M.S., Microbiology. Research Fellow, Rutgers University, New Brunswick, N. J.

Koffler, Anna H., Ph.D., Bacteriology, Immunology, Serology. Research Assistant, Bacteriological Development Department, E. R. Squibb and Sons, New Brunswick, N. J.

Lapan, Bernard, M.D., Gynecology and Obstetrics, Pathology, Endocrinology. Junior Adjunct, Obstetrics and Gynecology, and Volunteer Assistant in Pathology, Lebanon Hospital, New York, N. Y.

Larson, Nora L., M.S. Research Bacteriologist, Takamine Laboratory, Inc., Clifton, N. J.

Lehmann, Gerhard, Ph.D. Pharmacologist, Hoffmann-LaRoche, Inc., Nutley, N. J.

Mangieri, Carmen N., B.S. Head, Department of Pharmacological Control, Hoffmann-LaRoche, Inc., Nutley, N. J.

Meaney, Margaret Ann, B.A., Biochemistry. Pharmacology Research, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

Merck, George W. President, Merck and Company, Inc., Rahway, N. J.

Molomut, Norman, Ph.D., Bacteriology, Physiology, Biochemistry. Associate Director, Biological Laboratories, Brooklyn, N. Y.

Newell, Thomas W., A.B., Chemistry. Member of Commercial Development Division, Merck and Company, Inc., Rahway, N. J.

Novack, Lazare, Ph.D., Organic Chemistry, Biochemistry, Pharmacology. Research Fellow, Faculty of Medicine, New York University, New York, N. Y.

Port, William S., M.S., Organic Chemistry. Research Chemist, Heyden Chemical Corporation, Garfield, N. J.

Prier, James E., D.V.M., Veterinary Science. Veterinary Bacteriologist, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

Ross, O. A., Ph.D., Bacteriological and Biochemical Research. Research Biologist, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

Rubinstein, Michael A., Ph.D. Research Assistant in Hematology, Mount Sinai Hospital; Adjunct Attending Physician for Hematology, Montefiore Hospital; Instructor in Hematology, Post-graduate Courses, Columbia University, New York, N. Y.

Samuels, Leo T., Ph.D., Endocrinology, Metabolism. Head, Department of Biochemistry, University of Utah Medical School, Salt Lake City, Utah.

Seegal, Beatrice Carrier, M.D. Associate Professor of Bacteriology, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Sichel, F. J., Ph.D. Professor of Physiology, University of Vermont College of Medicine, Burlington, Vt.

Smith, Carl H., M.D., Hematology. Assistant Professor of Clinical Pediatrics, Cornell University Medical College, New York, N. Y.

Sosman, Robert B., Ph.D., Chemistry, Geology, Physics. Physicist and Assistant Director, Research Laboratories, United States Steel Corporation, Westfield, N. J.

Starr, Mortimer Paul, Ph.D., Bacteriology, Biochemistry, Plant Pathology. Assistant Professor of Biology (Bacteriology), Department of Biology, Brooklyn College, Brooklyn, N. Y.

Tager, Morris, M.D., Bacteriology, Medicine. Assistant Professor of Bacteriology, Yale University Medical School, New Haven, Conn.

Tamerin, Joseph A., M.D. Associate Surgeon in Charge of Plastic Surgery, Harlem Hospital, New York, N. Y.

Wagley, Charles W., Ph.D. Assistant Professor of Anthropology, Columbia University, New York, N. Y.

Watson, R. Janet, M.D., Hematology. Instructor of Medicine, Long Island College of Medicine, Brooklyn, N. Y.

Weintraub, Solomon, M.D., Pathology. Director of Laboratories, Harlem Hospital, New York, N. Y.

Wendt, Arthur S., Ph.D. Chief Chemist, Fred Fear and Company, Brooklyn, N. Y.

Woodcock, Alfred H. Research Associate in Oceanography, Woods Hole Oceanographic Institute, Woods Hole, Mass.

ASSOCIATE MEMBERSHIP

Caldwell, Mary L., B.S., Microchemistry. Research Assistant, Research Laboratory, General Electric Company, Schenectady, N. Y.

Francis, William J., Jr., B.Sc., Meteorology and Oceanography. U. S. Navy, Aerological Officer, Naval Air Station, Lakehurst, N. J.

Gall, Lorraine Sibley, Ph.D., Bacteriology, Nutrition. Research Fellow, Nutrition Laboratory, Yale University, New Haven, Conn.

Gordon, William E., M.S., Microwave Radio Propagation, Meteorology. Chief Meteorologist, Electrical Engineering Research Laboratory, University of Texas, Austin, Texas.

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Strauss, Ulrich Paul, Ph.D., Physical Chemistry of High Polymers. Research Fellow, Department of Chemistry, Yale University, New Haven, Conn.

Wexler, Raymond, M.S., Meteorology. Meteorological Physicist, Evans Signal Laboratory, Belmar, N. J.

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STUDENT MEMBERSHIP

Vogel, Herbert, B.S., Psychology. Student, New York University Graduate School, New York, N. Y.

Weiss, Sidney, M.S., Chemistry. Student, Fordham University, New York, N. Y.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 9 MARCH, 1947 No. 5

SECTION OF PSYCHOLOGY*

FEBRUARY 17, 1947

DOCTOR DONALD W. MACKINNON, Bryn Mawr College, Bryn Mawr, Pennsylvania: *Some Problems of Assessment.*

For twenty months—from December, 1943, until the end of hostilities in August, 1945—a group of psychologists and psychiatrists in the Office of Strategic Services was charged with the task of judging the suitability of candidates for various specialized and hazardous assignments overseas. In performing its duties, this group developed a set of techniques which have become known as the procedures of assessment. It is assessment as a method for selecting highly skilled personnel for positions of authority and responsibility, assessment as a research technique for the study of the development, structure, and dynamic functioning of personality as well as for the investigation of interpersonal and intergroup relations, and certain problems which arise in the use of assessment procedures, that I wish to call to your attention.

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* The Section of Geology and Mineralogy held a meeting on February 3, 1947, at which Doctor Robert B. Sosman, Research Laboratory, United States Steel Corporation, Kearny, N. J., presented a paper on *Some Geological Phenomena Observed in an Iron and Steel Plant.* No abstract of this paper has been received.

The Section of Biology held a meeting on February 10, 1947, at which Doctor Raymond Libby, American Cyanamid Company, Stamford, Connecticut, presented a paper on *The Use of Tagged Antigens in Immunochemical Studies,* illustrated by lantern slides. An abstract of this paper will be published in a later issue of TRANSACTIONS.

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Assessment, as a method for the study of man, attempts a reconciliation of opposed approaches to problems of personality. It seeks, on the one hand, to develop maximally clear, idiographic representations of individuals and, on the other, to discover nomothetic traits or variables of personality, relevant to whatever the purpose at hand may be, which make possible a comparison of individuals within a single frame of reference.

The dual nature of assessment as practiced in the OSS may be seen in the two-fold Final Report which was rendered for each candidate. The face sheet of the report consisted of ratings on a six-point scale (very inferior, inferior, low average, high average, superior, very superior) of ten variables of personality, the first seven of which—motivation for assignment, energy and initiative, practical intelligence, emotional stability, social relations, leadership, and security—were considered important for all candidates, while the last three—physical ability, observing and reporting, and propaganda skills—were relevant only for certain types of assignment. The main part of the report consisted of an Evaluation Note or personality sketch of about seven hundred and fifty words. The purpose of this note was to give a picture of the personality structure of the candidate in the light of which predictions of his performance overseas might be made.

The procedure of assessment which led to these reports required that a group of candidates, usually 18 in number, live for three and a half days in an assessment area known as Station "S," a hundred-acre estate fifteen miles from Washington, D. C. There, candidates lived informally while being entertained and examined in a variety of ways by the assessment staff.

Throughout the period of assessment, the identity and background of each candidate was unknown to the others. For the most part, the identity of each man was also hidden from staff members, the true facts of the candidate's life being revealed only under certain conditions, *e.g.*, in the privacy of a personal-history interview. Before coming to the area, each candidate was told to assume a pseudonym and, after having been stripped of all clothing that might reveal his rank or station, he was issued Army fatigues. Upon arrival at "S," the first task assigned each man was that of developing and maintaining as consistently as possible, throughout his stay in the area, a fictitious story of his life that would hide his true identity. Thus, there was created for the candidates at Station "S" a society in which, after all previous social distinctions had been obliterated, each could choose his own personality and, in that role, be as effective as his abilities would permit him. The task of developing and maintaining a fictitious story of one's life proved to be an extremely interesting projective test, revealing, among other things, a man's intellectual resourcefulness, the content and strength of his ego-drives, and even something of his integrity. With every man's identity hidden from his associates, and with each deprived of his accustomed emotional supports as well as his customary social position, the situation of living at "S" was one in which the place in the group won by each man and the interpersonal relations established between all of them were largely determined by the intrinsic qualities of the candidates. Such conditions are almost ideal for investigating not only the dynamics of individual action but also the dynamics of group formation.

In order that the personalities of our candidates might be intensively studied, each class of 18 was broken into three subgroups of six. To each of these subgroups, a team of three staff members was assigned, its special responsibilities being to interview the six candidates, to observe and rate them in all situations, and to prepare the final report on each of them.

During the three and a half days of assessment, each candidate participated in some 35 procedures and, on the basis of his performance in them, received over 75 scores and ratings. The multiform techniques of assessment included:

Standardized paper-and-pencil tests of personality, intelligence, and special abilities.

Projective tests to uncover repressed tendencies and to disclose aspects of personality which candidates could not or would not reveal.

Individual situational tests in which a candidate had to solve a problem alone.

Group situational tests in which groups of four to seven candidates had to solve a problem, sometimes with the leadership of the group assigned to a member, at other times with no leader designated.

An obstacle course to test physical fitness.

A personal-history or psychiatric interview in which an attempt was made to discover the dynamic structure of the candidate's personality. In preparation for this, the interviewer looked over a personal-history form previously filled out by the candidate and also reviewed his performance on the objective and projective tests.

Psychodramas tailor-made for each candidate. The candidates were cast, usually two at a time, in such a way as to force them to reveal in their actions suspected but, up to that time, unconfirmed strengths or weaknesses.

A debate on a timely topic, held on the next to the last evening, with hard liquor for all who wanted it. This convivial evening usually lasted long past midnight and often revealed aspects of behavior not shown in other situations.

A sociometric questionnaire which was given to all candidates toward the end of the assessment period.

A judgment-of-others test in which all candidates were asked to write personality sketches of five of their associates.

Another, and by no means the least important, source of information were the observations made and insights gained by the staff in their intimate day-to-day living with the candidates.

It must be emphasized, at this point, that the most characteristic aspect of assessment was not this multiplicity of techniques but rather the principle which determined every phase of the program: that the personality of each candidate be studied as an organized dynamic process.* Though, of necessity, many of the tests which were used revealed only part-processes and segments of the personality, the direction of assessment was away from the elementaristic approach to the study of personality and toward a holistic or organismic conceptualization of the person. Both the temporal arrangement of procedures in the program and the division of labor among members of the staff were determined by our desire to create a situation in which the organizational factors in the personality could be most effectively grasped. To this end, members of the staff were made responsible for persons, not for procedures; as members of staff teams, they observed a few men in all situations rather than focusing their attention upon the behavior of all candidates in a few situations.

The principle of studying personality as an "organized whole" determined the way in which all data were utilized and the final results of our assessment of candidates ordered and presented. A constant attempt was made to achieve an interpretative synthesis of all the data on each candidate. There was never any thought that a mere addition of test scores would yield the picture of personality as an organized whole which was our goal. In seeking to find the essential structure of a candidate's personality, we attempted to uncover its most general and significant individual patterns and trends, and to follow their ramifications in a wide range of the person's behavior. The more of his observed behavior we were able to relate to such general patterns, the better we thought we understood him.

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* For the particular formulation of the principles of assessment which are presented here, I am indebted to Dr. Eugenia Hanfmann, who served as a member of the OSS assessment staff.

The task of discovering the most general significant patterns and trends of an individual personality requires that one be ever on the watch for the possible interrelations of all observed behavior. One constantly has to interpret and reinterpret each observation in the light of all others and on the basis of those tentative hypotheses to which earlier observations have already given rise. Such a process of discovering the structure of a personality involves a weighing and integrating of data which is basically different from a mere summation of the results of separate performances.

The process of arriving at an understanding of an individual personality through a series of approximations, culminating in a consistent picture of a differentiated whole, though it is the very core of assessment, cannot be considered as peculiar to it. This process is all but unavoidable in all personality study and is utilized, with varying degrees of effectiveness, by everyone who engages in this pursuit: by psychologists and psychiatrists as well as by the layman who employs it uncritically and, frequently, with poor results. More often than not, however, the agent in this process is not fully aware of its nature, nor of its implications for the conception of human personality. Thus, psychologists, in describing their methods of investigation, tend to concentrate not upon this process, but upon the specific techniques used to obtain data, inasmuch as these are more palpable and quantifiable. By neglecting to recognize the nature of these intermediary but basic operations, they forfeit their chance of utilizing them fully, as well as their chance of reducing the sources of errors inherent in them by well thought-out controls.

One of the most significant aspects of the assessment program in the OSS was the fact that it contained a recognition of the necessity of relating all of its data to each other, not in a mechanical way, but by an interpretative process directed toward the discovery of general patterns. Indeed, this principle was the framework within which the operations of assessment were performed.

The basic principle of assessment was implemented and, at the same time, protected from sources of error, through the participation of every member of the staff in all phases of the work. In so far as errors of insight and generalization are due to the personal biases of individual observers, the best correction seemed to us to lie in the pooling of all observations and interpretations. It was for this reason that the staff members worked in teams and that the results of all procedures, as soon as they were obtained, were made available to the whole staff. This usage provided each staff member with the widest possible range of facts for discussion and interpretation. Special times were provided in the program for group discussions of the candidates in which interpretations of observed behavior were exchanged and consolidated. This process of forming and revising hypotheses on an increasingly wider basis of data was carried out not only in discussions by the staff teams but also in the informal exchange of observations and opinions. It was this continuous search on the part of the entire staff for the personal equation of each candidate, with its correction of biases and gaining of new insights, that represented the main challenge of assessment and gave zest to what otherwise might have been a mere dreary repetition of routine procedures.

The principle of group discussion and group decision, as distinct from a mere mechanical process of averaging scores or counting votes, presupposes the possibility of arriving at a common conception of a given personality. To the extent to which this common conception succeeds in encompassing and meaningfully combining the various aspects of personality seen by individual members of the group, it can be accepted by all of them and can be considered as more valid, more adequate, and less subjectively biased than conceptions arrived at by researchers working alone.

Assessment based upon such principles is, however, expensive. There can be no doubt of that. The question which will be asked, and which certainly should be asked, is whether such assessment is more effective than simpler programs based upon elementaristic principles. Further, one will want to know whether the predictive efficiency of a three-and-a-half-day program of assessment is greater than that of a one-day program or that of a single interview. We do not as yet know the answers to these questions.

What is needed is an experimental design for assessment which will permit questions of this kind to be answered. But what is to be the nature of that design? Since evaluation notes cannot be treated quantitatively, as they yield neither scores nor ratings, it is reasonable to predict that one proposal will be to ignore the personality sketches. Some psychologists may say that they do not care whether evaluation notes are written or not; all they want are scores and ratings with which they can work statistically. Such an attitude reveals, on the one hand, wishful thinking and, on the other, a profound misunderstanding of the nature of assessment. For years, psychologists and psychiatrists have followed that will-o'-the-wisp, the belief in the ultimate discovery of a simple test that will reveal a personality completely or will be a perfect predictor of success on the job, even the most complex of jobs. Assessment holds out no hope that such a test will be found.

As for ratings, they have a value and a place in the study of personality, provided they are made on the basis of a sufficient body of observations. There is this difference between the making of ratings and the writing of an evaluation note: It is unthinkable that a psychologist would write a sketch of the structure of a man's personality without first knowing a considerable amount about him, but, unfortunately, it is not unthinkable that, in the absence of such complete knowledge, he would venture to rate the same man on a few traits. The tragedy of this, for research, is that once a number—any number—has been recorded, it is almost certain to find its way into the statistical hopper. One knows when a sketch of a personality makes no sense and can, consequently, discount or reject it, but a number carries no psychological content and, therefore, once it has been recorded as a rating, there are no criteria by which its psychological meaningfulness can be tested.

To be maximally meaningful, ratings must be made as part of the process of seeking unifying patterns and significant trends of personality. Their making must be embedded in the continuous attempt to work out an interpretative synthesis of all the data which the process of assessment yields. Only in this way can a real integration of the nomothetic and idiographic methods be achieved.

The psychologist set to make ratings can never focus his attention solely upon the person he is rating. Instead, he must compare the traits of this person with those which he has observed in the individuals of the reference population. On the other hand, the psychologist set to write an evaluation note must grasp the organization of psychological processes which gives the personality under investigation its distinctive structure. To reconcile these two approaches to the study of personality is not easy, and the attempt to do so at once raises some extremely interesting problems.

The psychologist engaged in rating variables of personality is usually motivated by at least three concerns: (1) that he use the entire range of his rating scale; (2) that his ratings approximate the normal curve; and (3) that his ratings be made with reference to a constant, unchanging population. The fulfilment of these conditions is not easy, and the assumptions underlying them are, in many cases, questionable.

One might seek to meet the first two requirements, as we did in the OSS, by repeatedly reminding raters that they should use the whole scale and that their ratings should, over a sufficiently long period, be distributed somewhat as follows:

Group					
0	1	2	3	4	5
Very inferior 7%	Inferior 18%	Low average 25%	High average 25%	Superior 18%	Very superior 7%

If those coming to assessment are not differentiated from the total population in respect to the traits to be rated in the assessment program, the total population can be taken as the reference population in comparison with which the assessees are to be rated. Presumably, under these circumstances, it should not be difficult to fulfil, simultaneously, the two conditions that all steps of the rating scale be employed and that the actual ratings approximate a normal distribution.

Actually, in the OSS, our problem was complicated by the fact that our population of assessees was highly selected. It is extremely likely that, in most programs of selection and assessment, the population to be studied will be selected on the basis of certain of the variables to be rated. To the extent that this is so, if the reference population is the total random population, the ratings made in assessment of traits involved in the initial selection of a candidate will be skewed. Or, if they are not, the raters will have used another reference population. Indeed, there is reason to believe that, in the course of time, the raters will, in any case, come to use another reference population, for reference populations exist only in the minds of raters, where they are constantly being changed to the extent that subjects already rated have deviated from the norm on traits rated.

Let me illustrate from our experience in the OSS where, despite our most conscientious and conscious efforts to make our ratings approximate the rough standard already described, we were never able to adhere to it very closely. We came nearer to our goal on some traits than on others, but we did not reach it on any. However, the significant fact is that the direction of our failures was not constant throughout the twenty months of assessment. In general, in the early months of the assessment program the distribution of ratings showed a negative skew (in other words, we were rating high), while in the later periods of assessment the distribution of our ratings was positively skewed (we were rating low).

It would be simple and certainly gratifying to our egos to assume that our ratings merely reflected a true picture of the relative strength of the variables in our early and late assessment populations, that the candidates who came early to assessment were by and large superior to those who came late (at the time when one so often heard comments about the scraping of the manpower barrel). It seems likely that there was, in truth, a difference between our early and our late groups, but it is doubtful that it was as great as the difference in our early and late ratings would imply.

Part of this difference is, I suspect, to be explained by shifts in the scale values of the frame of reference against which members of the OSS staff made their ratings. One can rate only with reference to populations which one has known, and for the majority of staff members the populations best known and most thoroughly studied prior to their experience in the OSS had been, for the psychologists, college and university students and, for the psychiatrists, their patients. Now, in assessment, they were asked to rate the suitability of candidates who had been picked for assessment because their accomplishments had already shown that they belonged, for the most part, in the middle and upper ranges of the distribution curve of general effectiveness. It is understandable, then, that candidates assessed in the early days of the program should have been perceived by the psychologists and, especially, by the psychiatrists, as an unusually superior group. The negative skewness of our ratings indicates that this is precisely the way in which we viewed them. It would have been strange, indeed, had our experiences in the early days of assessment not created, in our minds, whether we wanted them to or not, new standards and new values for the steps of our rating scales. After the superior population which we first assessed had established, in our minds, a new reference population, which was the more impressive and compelling because of its contrast with the frame of reference with which we had entered upon the work of assessment, it was not surprising to find a general lowering of our ratings in the later months of assessment. But to what extent the later ratings were depressed because of the poorer quality of candidates then being assessed, and to what extent they were depressed because of the shift in our scale values, is impossible to say. It is important, however, to stress the fact that, in any long continued assessment program, regardless of what the purpose of the program may be, the problem of the shift in scale values must be faced. The problem will be magnified to the extent that the population to be assessed deviates from the population of subjects previously studied by the raters.

This problem, it should be noted, is not peculiar to the rating of personality traits. It has its counterpart in the most varied kinds of judgmental situations in which the importance of frames of reference and standards has been demonstrated, *e.g.*, in the making of psychophysical and affective judgments and in the formation of attitudes and opinions.

The implications of this discussion for the diagnosis and assessment of personality should be clear. Though the desire to use the entire range of one's rating scale is both understandable and commendable, the device which is frequently employed to achieve this end, and which we employed in the OSS—that is, the attempt to make ratings approximate a normal distribution—is, to say the least, questionable. We may well ask to what extent the trait of leadership is normally distributed in *any* population, and the same sort of query may be made with considerable cogency with respect to many other rated variables of personality. Actually, as we have seen, it is difficult, if not impossible, for a group of raters working over a long period of time to distribute their ratings in accordance with some proposed pattern, for reference populations have a way of changing in the minds of raters. One consequence of this is a shift in scale values—sometimes conscious, but perhaps more often unconscious—which introduces a skewness of ratings and makes it impossible to compare the ratings given candidates at widely different times in the same program. Thus, we come to the conclusion that ratings, frequently urged and justified on the grounds that they serve the end of nomothetic research by making possible a comparative study of individuals, actually serve this purpose less well than does the method of personality description which focuses attention upon the structure of the individual as an organized dynamic process.

Despite these strictures, ratings will, of course, continue to be used widely in personnel selection and in personality research. The above remarks are not intended to imply that ratings have no place in the study of personality. They have a very real place when they are based upon an interpretative synthesis of data derived from many sources. Unless they are so founded, they have nothing to add to the scores obtained by subjects on a series of discrete tests. In effect, this is to say that ratings should be made in conjunction with the discovery of the dynamic personality structure of the individual whose traits are to be rated. But at what points in such an integrated program of personality assessment are ratings to be made, and at what point is the final sketch of the personality to be written and discussed by the staff?

The requirements of a neat experimental design might decree that all ratings, both individual and pooled, be made before the evaluation note is written and discussed by the assembled staff. The contamination of ratings by information from sources other than those approved by the design is, of course, something which it is the purpose of the design to prevent. However, a design which achieves purity of information at the expense of adequacy of assessment is one which fails in its primary aim.

Already, some evidence is at hand to suggest that it is not a matter of indifference whether final ratings of variables are made before or after the personality sketch has been written, though the precise nature of the influence of ratings upon sketch or sketch upon ratings remains a matter for research.

It has already been indicated that, in the OSS program, each class of 18 candidates was divided into three subgroups of six and that to each of these subgroups a team of three staff members was assigned, to observe and rate them in all situations and to prepare the final report on each of them.

In each situational test, the members of a staff team rated independently the performance of all six candidates whom they observed, but the final rating of each variable for each candidate in the situation was not reached by a simple averaging of the ratings given by the three observers. Rather, after each situational test a team conference was held to discuss the observed performance of each candidate and the meaning of his behavior in the context of what had been learned about him in previous situations and tests. In the light of this discussion, a final rating of each variable observed in the situation was agreed upon for each candidate. Sometimes, it was an average of the ratings given independently by the three staff members; at other times this was not the case. Throughout any one period of assessment there was, then, a constant interplay between the discussion of the increasing insights which were being gained concerning the personality of a candidate and the ratings which were given him in the different situations.

On the morning of the last day of the assessment of each group of candidates, members of staff teams met in conferences to discuss their candidates fully, before withdrawing to write the evaluation notes for the final reports. In these conferences, all data collected in the assessment of each candidate were discussed, but final ratings were not decided in these meetings. They were later determined in conference by the entire staff after they had heard and discussed the evaluation note on a candidate. At this time, the staff had the benefit of a huge scoreboard at one end of the conference room, on which the final rating of each variable in each test or procedure for each candidate was represented by a colored thumb tack—red ones for ratings above average, blue ones for ratings below average. The determination of the final ratings to be recorded on the candidate's rating sheet was largely a matter of averaging the separate ratings of each variable as indicated on the board, *provided* these averages were congruent with the picture of the candidate as given in the evaluation note. When the average of the ratings of a variable for a given candidate did not agree with the implications in the personality sketch concerning the nature and strength of the variable, our task, as we referred to it jokingly, was that of "homogenizing" the report. The changes which were made in order to achieve this homogenization of the two parts of the report were decided upon by group discussion and decision. Sometimes they involved a change in wording or the rewriting of a phrase or paragraph in the personality sketch, though more often the congruence of the two parts of the report was achieved by deciding upon final ratings of variables other than those we would have arrived at by a simple averaging of subratings.

Why, one may ask, were ratings changed to conform with the meaning and implications of the evaluation note more often than notes were rewritten to bring them into line with the ratings? The answer would seem to lie in the fact that a written sketch of a man's personality possesses more structured meaning than do ratings which have been assigned to certain variables of personality. Of course, in the OSS program, changing a rating meant deciding upon a rating other than the one which would have resulted from a mere averaging of subratings rather than changing a final rating upon which agreement had already been reached by the staff. But even had such final pooled ratings been determined before hearing the evaluation note, there is reason to believe that ratings would still have been changed more often than the personality sketches have been rewritten. This is not simply the result of inertia. It has little to do with the fact that it takes less effort to write a new number than to rewrite a paragraph. It is rather, I am convinced, a function of the fact that one has less feeling of doing violence to a picture of personality when one changes a rating than when one modifies a sketch of personality. There is greater congruence between one's impression of a man's personality as an organized dynamic process and a written sketch of that personality than there is between that impression and the one conveyed by a set of ratings. This fact alone suggests the greater appropriateness of a personality sketch, as against ratings, for conveying to others an impression of a personality.

Members of the OSS assessment staff were often of the opinion that they had not grasped fully the personalities of the candidates whose reports they were preparing, until they had completed the process of setting down in words the impressions of their subjects which they had formed and reformed in the course of assessment. Indeed, it was often our feeling that some of the most enlightened integrations of impressions came in the very process of formulating in words the personality structures of our candidates, for then we were concerned to convey to others that understanding which we had gained of personalities as organized dynamic wholes.

The skeptic may say that the sense of closure which one experiences in successfully putting into words, on paper, a picture of a man's personality, results only from the fact that, in doing so, one has been able to indulge one's urge to be consistent and complete. He may ask whether there is any reason to believe that a picture which possesses more meaning and better composition is necessarily a truer picture than one which is less meaningful and more poorly composed. May not the very richness of detail in a sketch of personality convey a false impression of verisimilitude? Nothing can be said other than that this is a problem for research.

The surprising fact in the preparation of our reports in the OSS was not that the two parts of the final report sometimes had to be homogenized, but that they so seldom lacked congruence. The reason for this, undoubtedly, lies in the way ratings were made an integral part of the process of assessment.

This year, I have participated in two new programs of assessment. In both of them, an attempt has been made to fit procedures into an experimental design involving a considerable separation of the process of rating from that of preparing the personality sketch. The design has even required that information received in the interview be kept from staff members until ratings have been made in all situational tests, and that all final pooled ratings be determined prior to the writing and staff discussion of personality sketches. In both of these programs, trait ratings have frequently appeared to be in conflict with the connotations of the personality sketches. This observation raises a number of questions and suggests some interesting researches:

Is it a fact that ratings convey a more adequate picture of a personality when they are made as part of the process of interpretatively synthesizing a large body of data than when made on the basis of discrete scores and observations?

Is it possible, on the other hand, that the congruence between ratings and personality sketch when they are made conjointly is an artifact, the expression of an urge to be consistent?

Asch's study of forming impressions of personality[1] has implications for our problem. There is reason to believe that more attention than in the past should be given to the order in which trait-names are printed on a rating sheet. Here is another problem for research: to investigate the influence of different arrangements of trait-names upon the impression of the personality which they convey to the reader. And, if the order in which trait-names are listed may alter markedly the impression of personality which they convey, then, as Asch has shown, serious attention must be given to the order in which reference is made to traits in a written sketch of personality.

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[1] Asch, S. E. Forming impressions of personality. J. Abn. & Soc. Psychol. 41: 258–290. 1946.

These are but a few of the problems in need of investigation in the area of assessment. It is to be hoped that experimental designs which will permit their solution may be devised. Not until the problems which are central to the process of forming adequate and valid impressions of personality as an organized dynamic process are experimentally investigated will the full promise of assessment be realized.

SECTION OF ANTHROPOLOGY

February 24, 1947

Mr. Edward F. Haskell, Department of Sociology and Anthropology, Brooklyn College of the City of New York, Brooklyn, N. Y.: *A Natural Classification of Societies.* (This lecture was illustrated by lantern slides.)

For every science, natural classification is the general mobilization of data. It is the prerequisite of the science's advance out of the first stage (the long period of simple data collection, or description) into the third, that of evolution theory. Thus, the work of natural classification comprises the second of at least four development stages through which all sciences progress.

For biology, the system invented by Linnaeus ushered in a century of intensive classification of organisms. But his binomial taxonomic series, not being a fully natural classification (see below), has not made possible an adequate theory of organism evolution to this day. For physical science, Mendelyeev's periodic table gave rise, a generation later, to Rutherford and Soddy's theory of transmutation, of atom evolution. For social science it will, I hope, be the present paper which will provide the mobilizing mechanism called *natural classification.* Whether this can eventually lead to adequate society-evolution theory or not, may also be judged from what follows.

Two conditions have, thus far, been laid down for natural classification; the first by John Stuart Mill,[1] the other by Talcott Parsons.[2] The first is that the classified property must be related to all the other properties of the classificand, so that its classification involves theirs, too. (For atoms, Mendelyeev discovered this property to be atomic weight.) The second condition is that the constructs in terms of which the classification is stated themselves be amenable to mathematical manipulation. (Moseley's atomic numbers, a refinement of classification by atomic weight, lend themselves to consecutive counting.) It is because the binomial taxonomic series fulfils neither of these conditions that it is not a natural classification in the technical sense of the term.

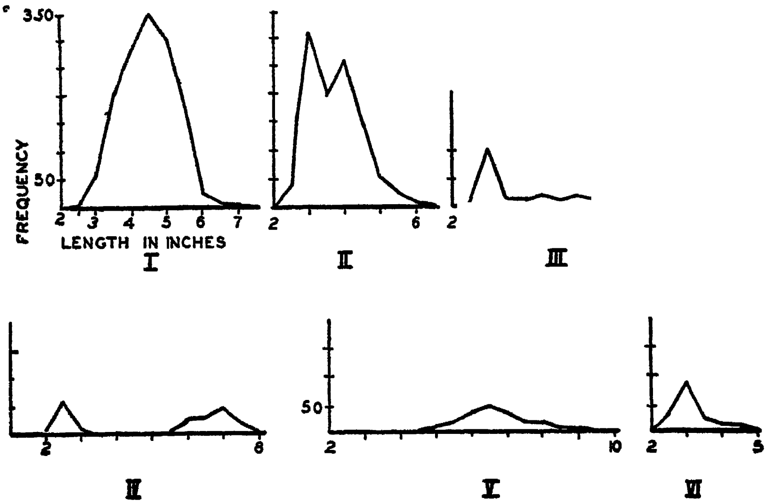
The strategic property to be classified I found in a series of observations of societies formed by bass (fish), and reported by T. H. Langlois of the Ohio Department of Agriculture.[3] I discovered one property which changed regularly, and with whose changes all the other properties were transformed. For that property, I have borrowed Clements' and Shelford's term, *coaction.*[1] Coaction occurs in any life process (nutrition, reproduction, protection, etc.) whenever the strong individuals either increase or decrease this process' normal rate in the weaker ones, or *vice versa,* or both.

In these bass societies, a simple case with which to begin, coaction started out positively. At first, the powerful bass probably increased the feeding of the weak by giving them a good example which they followed, they themselves being neither helped nor hindered by the weak (FIGURE 1, I). Soon, however, they began to chase the weak bass away from the food, thus decreasing their nutrition, though remaining unaffected themselves (II). Then they began eating the smaller bass, thus decreasing their nutrition while the latter increased the nutrition of the big bass (III). Finally, the small bass organized themselves into a separate school, systematically fled and hid, thereby increasing the work and decreasing the nutrition also of the large, thus producing two-sided decrease (IV). We have, then, four different coactions.

The important thing is that, with each change of coaction, all the other properties of the bass societies changed as well. The shape of the curve obtained by plotting body size changed, as shown in FIGURE 1. This means not only change of the distribution of power (1) in these societies, but also of work (2) and consumption (3). The cohesiveness (4) of these societies also changed, as did the behavior (5) of the individuals. (A well-integrated school of relaxed individuals changed first into a school of diversely feeding, tense individuals, then began to disintegrate and finally split into a school of predators and a school of prey.) The productivity (6) of the societies changed regularly, too. When deprivation appeared, productivity declined; dropped to a small fraction where predation had appeared and, where struggle developed, productivity was actually negative, less pounds of bass coming out of the pond than were originally put into it. These experiments will be fully analyzed in a forthcoming book.

Apparently, coaction is for the classification of bass societies what atomic weight is for the classification of chemical elements. I shall show below that this holds also for societies formed by other organisms, including man.

Langlois, however, showed only four coactions. How are we to determine the number and nature of all actual and also all possible co-



LANGLOIS' POWER DISTRIBUTION CURVES

FIGURE 1.

actions, as Mendelyeev and Mosely did for all actual and possible elements? Whatever happens to the less powerful and to the most powerful individuals we translate into common mathematical terms: *the normal rate of organismic activity we translate in 0, increase* thereof into +, and *decrease* into —. By the process of cross-tabulation, we get all actual and all possible coactions, as follows:

	−	0	+
+	−+	0+	++
	−0	00	+0
	−−	0−	+−

We must now proceed from Langlois' experiment to the great mass of data about societies formed by any and all species of plants, animals, men, and combinations thereof. The coaction in which the large bass ate the small is called 'predation.' This word, *predation,* belongs to a large class of terms in plant[5] and animal ecology,[6] anthropology,[7] and even—not to ignore the Eastern branch of the United Nations—to historical materialism.[8] The terms in this class, to which also belong *parasitism, commensalism, epiphytism, exploitation, symbiosis, cooperation,* etc., were developed by diverse people in various fields. Naturally, therefore, when they are brought together as a system, they do not fall into place neatly and completely like the cross-table generated terms. A great deal of overlapping, duplication, omission, and other logically inadmissible conditions appear.

In all my search, no conventional term has been found which does not or cannot logically be made to correspond to one of the nine mathematical coaction designations. Until such a term is found, our table must, therefore, be considered complete. From this, it follows that this cross-table makes possible systematic redefinition and organization of the basic terms of social science, yielding a very wieldy yet complete and universally applicable nomenclature:

Neutrality	0 0
Symbiosis	+ +
Commenselism	+ 0
(No name found)	0 +
Self-sacrifice	+ −
Parasitism	
Deprivation	− 0
(No name found)	0 −
Predation	− +
Struggle	− −

Such a vocabulary is one of the indispensable prerequisites for the development of systematic social science.

This process may seem arbitrary. However, W. V. Quine has described the same procedure as occurring very slowly and unsystematically in physical and biological science.[9] In fact, it is not only a normal but a necessary process in the development of every science. The increased system and speed of this process in social science are due to the unconscious accumulation of experience in the more advanced sciences, a general process described and named by Bateson,[10] and to its specific formulation by Quine, a logician.

Though I cannot here go through the list, I will show the main classifications of societies participated in by man. The great anthropological classification of some 650 peoples compiled by Hobhouse, Wheeler, and Ginsberg[7] is based upon animal-man and plant-man coactions, upon so-called *heterotypic* coactions. (Being designated by the misleading term "material culture," the real nature of these societies has failed to penetrate our awareness.) The term, *heterotypic* *societies,* however, by forcing us to recognize them, bridges the traditional chasm, in our minds, between science about plant and animal societies and science about man societies. By completing these authors' unfinished classification at the top of their figure (FIGURE 2, left half)

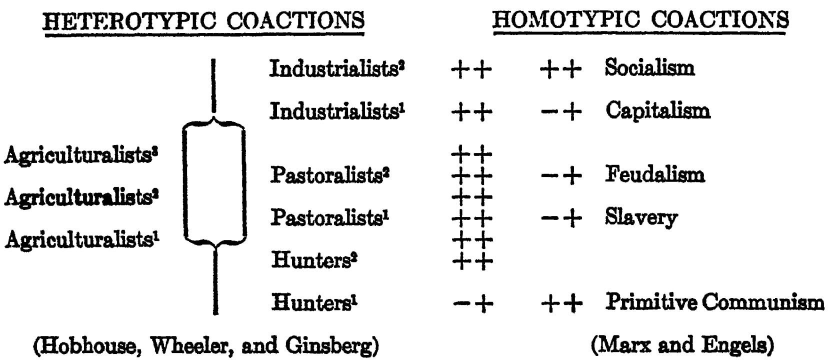

HETEROTYPIC COACTIONS
HOMOTYPIC COACTIONS
Industrialists^2 ++ ++ Socialism
Industrialists^1 ++ −+ Capitalism
Agriculturalists^3
Agriculturalists^2
Agriculturalists^1
++
Pastoralists^2 ++ −+ Feudalism
++
Pastoralists^1 ++ −+ Slavery
++
Hunters^2 ++
Hunters^1 −+ ++ Primitive Communism
(Hobhouse, Wheeler, and Ginsberg)
(Marx and Engels)

FIGURE 2.

I have, further, ignored the traditional barrier between anthropology and sociology. By adding the (Marxist) classification[8] of *homotypic* (man-man) human societies at the right, I have also ignored the traditional barrier between the school of social science, consciously based upon Hegel's philosophy, and that less consciously based on the philosophy of Francis Bacon.[*] That the two classifications coincide is demonstrated by many correlating graphs,[7] one of which is shown here as FIGURE 3. Other questions raised in FIGURE 2 are dealt with below.

Coactions occur in individual processes such as nutrition and reproduction. Hence, they can be and often are simultaneously of different sorts. To give each its correct value at every instant is a difficult and as yet unsolved problem. There is, however, an indicator of general, overall coaction which holds good, apparently, regardless of the species of organism forming the society. This is the power-distribution curve. Power for bass is body size. For primitive hunters, its is this quality, too, but implemented by high intelligence. Wherever coaction is 00 (or very mildly positive or negative) such power is distributed normally and approximates, when plotted, the bell-shaped curve. This is widely held to be true of 00 regardless of the organism

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[*] For a mathematical discussion, see *A Table of Natural Classifications* in a forthcoming issue of "Pallas," special international double issue of "Chronica Botanica."

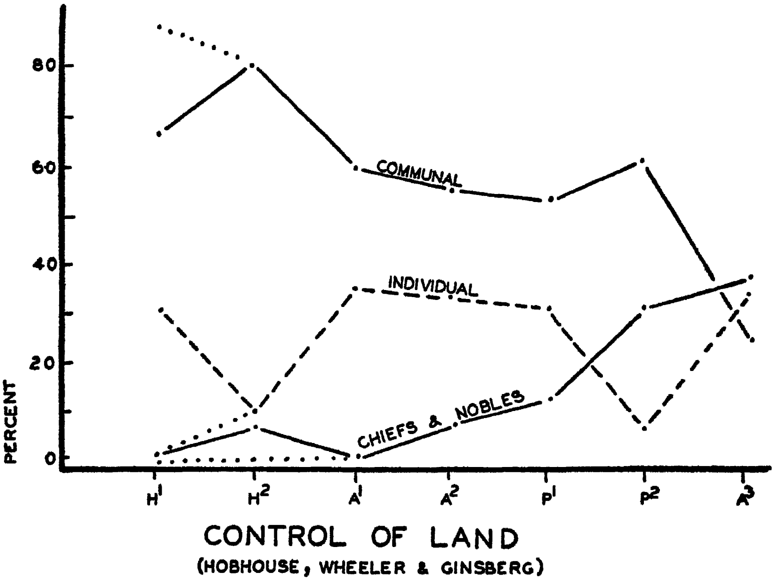


FIGURE 3.

in question. For mildly positive coactions, FIGURE 4 shows (at the top left) the size-distribution of bass. (Coaction, mildly + 0.) Below it is the probable distribution of production and consumption among primitive hunters, whose coaction is mildly + +. At the top right, bass body size distribution under — + coaction is shown. Below that, we see wealth distribution in a human society where coaction is — +.

To prove that this is our society's predominant coaction (as it is shown to be in FIGURE 2), we simply superimpose a bell-shaped curve on this figure, with its center at the median of the national income. There is no way for a people whose abilities are even approximately normally distributed to show so vastly skewed a distribution of consumption, except by some kind of predation. Whether, to complete discussion of FIGURE 2, 'socialism' is a form of symbiosis, + +, as also shown in FIGURE 2, or not, is purely a question of a term's definition. What a given supposedly socialistic country's coaction is, say the U.S.S.R.'s, I cannot tell until I have succeeded in obtaining its income-distribution curve.

These curves show that, in these cases, the main properties of

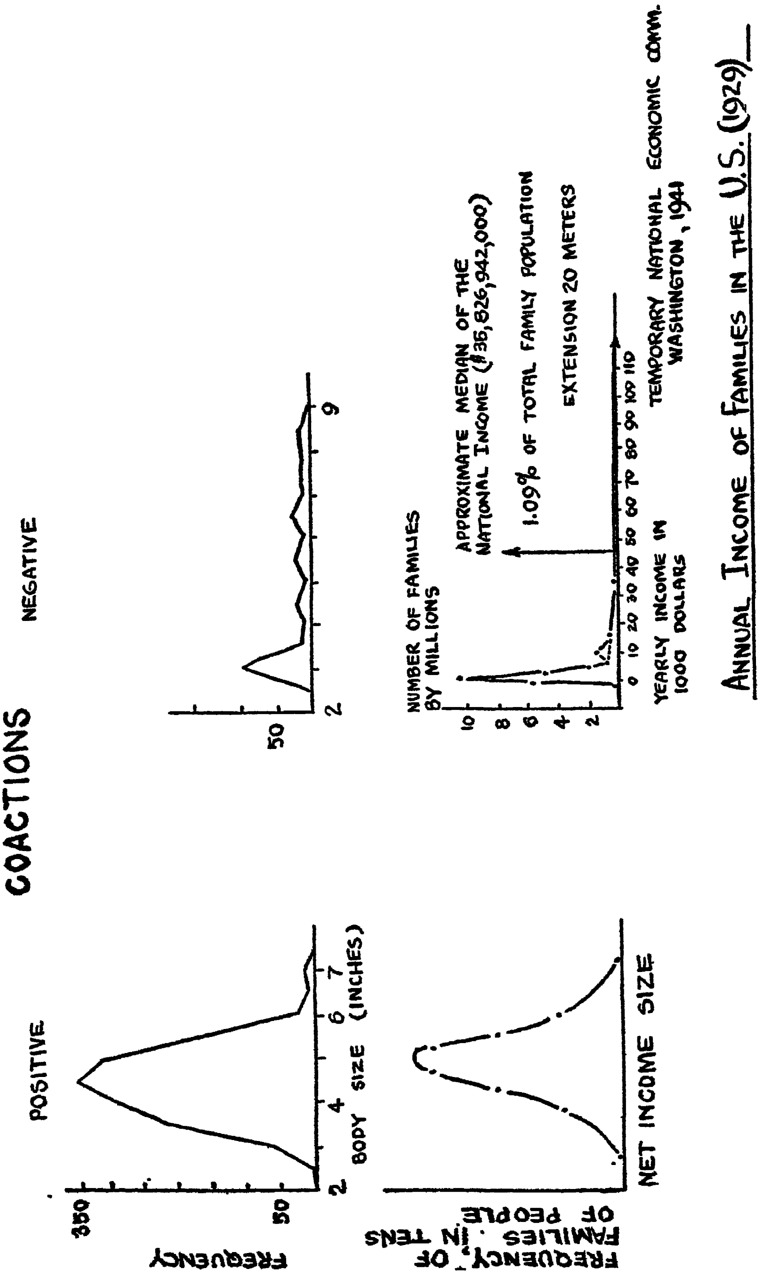
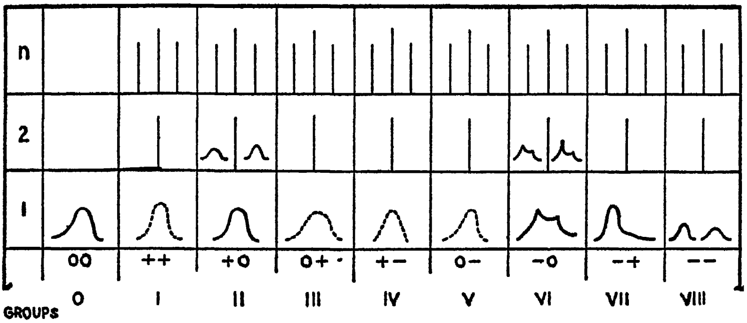

COACTIONS
POSITIVE
NEGATIVE
FREQUENCY
350
50
2
4
6
7
BODY SIZE (INCHES)
FREQUENCY OF FAMILIES IN TENS OF PEOPLE
NET INCOME SIZE
50
2
9
NUMBER OF FAMILIES BY MILLIONS
10
8
6
4
2
0 10 20 30 40 50 60 70 80 90 100 110
YEARLY INCOME IN 1000 DOLLARS
APPROXIMATE MEDIAN OF THE NATIONAL INCOME ($36,826,942,000)
1.09% OF TOTAL FAMILY POPULATION
EXTENSION 20 METERS
TEMPORARY NATIONAL ECONOMIC COMM. WASHINGTON, 1941
ANNUAL INCOME OF FAMILIES IN THE U.S. (1929)

FIGURE 4.

societies vary uniformly with coaction, regardless of the species of organism forming them. Many more cases, of course, are necessary for the establishment of this principle as a natural law.

Coaction is, apparently, a suitable criterion for the natural classification of societies. Let us now construct this classification. We have nine terms and, since, to the best of our knowledge, they constitute a totality, they must be the basis of our table. We also know, however, that there are far more than nine kinds of societies. It follows that our table must be periodic: and periodicity was discovered empirically by Langlois. In FIGURE 1, we see the end-products of schismogenesis,[11] V and VI. We are told, however, that they resemble I, and that they again go through the changes from I to II and may well go on to IV. (See also below.) Accordingly, I have made the coaction table (FIGURE 5) periodic.



COACTION TABLE

FIGURE 5.

This figure shows the changes of the power-distribution curves related to each of the coactions. As stated, the shape of each of these curves is probably constant, regardless of the kind of organism forming a society—plant, animal, or human—and regardless of whether their power is exerted by roots, leaves, and blossoms; teeth, claws, or fins; fields, herds, factories, pursuit planes, or atomic bombs. The observed curves are in solid lines, those merely deduced are dotted in.

FIGURE 5 permits us to ascertain theoretically all actual and all possible directions of society evolution, as we had expected it to do. This is most easily shown by way of vector notation, for this can show the quantitative variations of each coaction: Let a solid vector represent 0, a dotted rightward vector +, and a dotted leftward vector —.[12] Symbiosis is then represented as follows $\longrightarrow\overset{+}{\cdot\cdot\rightarrow}\longrightarrow\overset{+}{\cdot\cdot\rightarrow}$. (What happens to the weaker is shown at the left, to the stronger at the right.) Now let the right-hand acceleration decrease progressively until it disappears, thus: $\longrightarrow\overset{+}{\cdots}\longrightarrow{}^{\circ}$. This vector pattern represents commensalism. Then let the right-hand acceleration increase in a negative direction, thus: $\longrightarrow\overset{+}{\cdots}\rightarrowtail\!\longleftarrow\overset{-}{\cdot\cdot}$. This vector pattern represents parasitism or self-sacrifice. And so on for all nine coactions. They yield nine qualitatively different vector patterns. Obviously, any one of these (except 00) can display any number of quantitative variations. Moreover, it can, in so doing, proceed to change into any of the other eight coactions.*

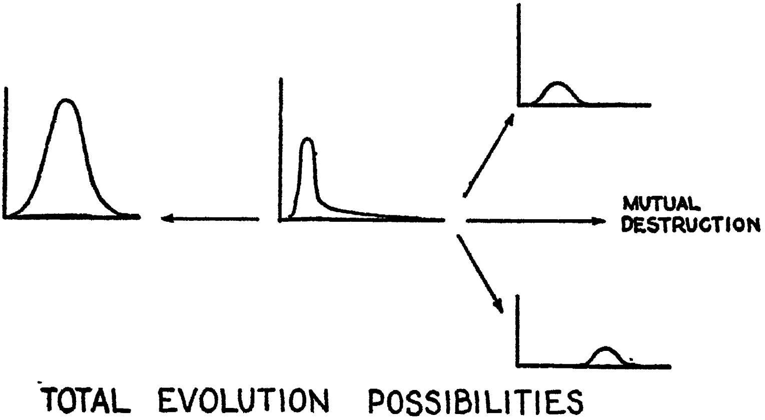


FIGURE 6.

The total possible directions in which our society can evolve are shown in FIGURE 6. The capitalist system is shown in the center. Comparison with FIGURE 1 shows that, in bass societies, predation goes over into struggle. Schismogenesis, originally discovered by Bateson for men[11] and provided for in Group VIII of the coaction table, FIGURE 5, may result in the total destruction of the weaker observed in bass and, in man, called *fascism;* of the stronger observed in bass by accident and, in man, called *proletarian dictatorship,* of both (by way, *e.g.,* of atom bombs), or of neither (*e.g.,* flight of the Israelites from Egypt).

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* The 00 coaction, too, can vary quantitatively, but its changes belong to biological theory. This coaction is the theoretical link between social and biological science, the latter of which can also be couched in terms of coaction theory (see below).

It is these alternative end results of schismogenesis (in FIGURE 1, V and VI) which are shown in the second period of FIGURE 5 and which, in their turn, begin to evolve. According to the Marxist theory of society evolution, these (with the exception of mutual destruction) are the total possible directions in which our society can change. One of the most important points of this paper, however, is the fact that, in bass societies, *reversal* of the original trend (transformation of predators into cooperators) was achieved by Langlois and, also, that it is being achieved for societies formed by men, as in Switzerland.*

Thus, we have again successfully ignored the traditional boundaries separating plant and animal ecology from anthropology, anthropology from sociology, and Baconian from Marxist social science. What has happened is that, as a result of natural classification, systematic social science is here. More than that, it is already in the third development stage, that of systematic evolution theory.

These developments were deduced, at the outset, from the general properties of natural classification. Now, however, there appear further possibilities which we did not expect: Due to the general (mathematical) nature of our constructs (*stronger* and *weaker* variables, +, 0, and —) coaction theory may possibly apply universally. One possible indication that it applies to atoms is the general isomorphism of the coaction table and the periodic table of chemical elements.† A possible indication that it does apply to organisms is the fact that coaction theory accounts for a number of general evolution phenomena. When natural classification of organisms occurs, it may thus be stated in these categories. If it should, science as a whole might commence to be unified.

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* These assertions are dealt with in a forthcoming book.

† For more detailed discussions, see the aforementioned article in "Pallas."

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SECTION OF PHYSICS AND CHEMISTRY

The Section of Physics and Chemistry has established a series of seminar meetings, which are to be held on the fourth Friday of each month of the remaining Academic Year, from February to May inclusive, under the chairmanship of Mr. J. J. Bikerman, Merck and Company, Rahway, N. J.

The program, as announced so far, will consist of the following papers:

February 28, 1947. Mr. A. S. Kenyon, Columbia University, New York, N. Y.: *Light-Scattering Properties of Monodispersed Sulfur Sols* (illustrated by lantern slides).

March 28, 1947. Mr. J. J. Bikerman, Merck and Company, Rahway, N. J.: *Tackiness and Adhesion* (illustrated by lantern slides).

April 25, 1947. Doctor Gilbert D. Miles, Colgate-Palmolive-Peet Company, Jersey City, N. J.: *Interfacial Phenomena Associated with Mixtures of Surface Active Materials.*

May 23, 1947. Doctor I. Fankuchen, Department of Chemistry, Brooklyn Polytechnic Institute, Brooklyn, N. Y.: *X-Ray Diffraction and Colloidal Aggregates.*

Abstracts of the respective papers which are read at each meeting are scheduled for publication in the corresponding number of *Transactions.* In their absence, title and notice of presentation will be inserted.

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SECTION OF BIOLOGY*

MARCH 10, 1947

DOCTOR R. L. MAYER, Research Laboratories, Ciba Pharmaceutical Products, Inc., Summit, N. J.: *Histamine and Antihistaminic Substances.*

Our paper on the anti-allergic properties of antihistaminic substances in reality covers three distinctly different fields, each of which is sufficiently important to justify separate discussion. It is impossible to deal with the action of antihistaminic substances without first discussing the object of their attack, namely, histamine; and it is not possible to discuss the use of these antihistaminic substances in allergic diseases without referring to the role that histamine plays in allergic diseases. Then, again, this talk would still be incomplete without mentioning those facts which prove that the so-called antihistaminic substances indeed counteract allergic manifestations because they neutralize histamine—*i.e.*, that there is a relationship between cause and effect.

We shall, therefore, become involved in a rather complicated triangle of three different problems which are not necessarily connected by short lines.

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* The Section of Geology and Mineralogy held a meeting on March 3, 1947, at which Doctor M. L. Brashears, Jr., presented a paper on *Geologic and Ground Water Conditions on Long Island,* illustrated by lantern slides. No abstract of this has been received.

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Most of our synthetic drugs have been developed from time-honored, effective, but crude medicaments. The new powerful synthetic antimalarial compounds go back to quinine; the newer organic mercury compounds, used in syphilis, have their precursors in the mercury vapors of medieval mirror factories; while salvarsan was developed by Ehrlich, who knew that Fowler's solution had a beneficial effect in certain protozoal diseases. Other present-day useful remedies have been found by pure accident, such as the barbiturates and saccharine, to mention only two. However, today, there exist a large number of important synthetic drugs which are the result of pure theoretical speculation or systematic experimental research. The antihistaminic substances are a good example.

PHARMACODYNAMICS OF HISTAMINE

As the name implies, the predominant property of antihistaminic substances is their ability to counteract histamine, that is, to nullify its toxic effects. The first question we shall attempt to answer is, therefore, "What are these actions of histamine which we want to counteract or nullify?"

Histamine is a regular constituent of the animal body and also a metabolite of certain bacteria and fungi. It is probably formed by enzymatic decarboxylation of histidine.

It is distributed throughout many organs with the white blood cells, the cells of the skin, the lungs, the liver, and the intestines being particularly rich in histamine. There are wide differences in its quantitative distribution. It seems more than fortuitous that the greatest amounts are found in the so-called border cells, that is, those cells which are in direct contact with the outer world. This may indicate that histamine plays a significant physiological role in the defense mechanism of the body against injuries coming from outside.

Dale and Laidlaw studied its pharmacologic effects in 1910 and found histamine to be a general stimulant of the secretion of various glands. It increases lacrimation and salivation and is one of the most potent stimulants of the gastric secretion. Furthermore, it strongly contracts the smooth muscles, especially those of the lungs, intestines, bladder, uterus, etc. The smooth muscle spasms produced by histamine are very intense: for example, histamine-induced spasm of the bronchial muscle of guinea pigs causes rapid death by suffocation.

In man and in certain animal species, histamine causes dilatation of the capillaries and venules and, thus, hyperemia and skin wheals. In cats, dogs, and man, the blood pressure falls rapidly. Histamine apparently plays an important role as a mediator for certain sensations in the skin or mucosas, especially pain and itching.

Not all animals are equally sensitive to histamine. There are, in fact, great differences in susceptibility, which is a phenomenon rarely observed with other compounds. The most sensitive animals are guinea pigs, dogs, and man. For guinea pigs, an intravenous injection of doses as small as 0.3–0.5 mg. per kg. causes death. Dogs are killed by 1–3 mg. per kg. Man also is sensitive to milligram amounts. The mouse, rat, and frog, on the other hand, are very resistant to histamine, and toxic symptoms occur only after receiving doses of 500 mg. or more.

The ubiquitous presence of such a powerful poison in the animal body is quite surprising. No doubt, it would constitute a constant threat for the very life of the individual, were not the body equipped with certain mechanisms through which to check its toxicity. The most important protection is afforded by the fact that, normally, histamine does not exist as nor circulate in its free, active form; actually, it is bound within the cells, apparently to amino acids like arginine or lysine, to form an atoxic and inactive compound. Moreover, the body contains an enzyme, histaminase, first studied by McHenry and Best, which is able to destroy, by deamination, free histamine should it circulate in excessive amounts.

We do not know whether, under normal physiologic conditions, a steady liberation of free histamine takes place. Under the influence of certain pathologic stimuli, however, the sudden liberation of toxic amounts occurs. One of the most general of these conditions is a cell injury, such as occur during burns, injection of toxins and animal poisons such as snake or bee venoms, the presence of certain parasites like *Trichinella* or scabies, chemical or physical irritations of the skin, etc.

Rocha e Silva has speculated on the mechanism by which histamine is released from its inactive form. According to his theory, proteolytic enzymes, contained in certain toxins or activated by injury of the cells, split the polypeptide-like linkages. As soon as enough free histamine, now in its active and toxic form, enters the circulation, it will produce its specific toxic effects, provided that the animal species is sufficiently sensitive to histamine. This is an important point: Histamine is liberated in the same proportions in either sensitive animals such as guinea pigs or in resistant animals like the mice. Toxic symptoms occur only in the former or sensitive group. In addition to the abovementioned types of cell injury, one of the most important factors responsible for the liberation of histamine—important at least for the present discussion—is the combination of antigen and antibody as it occurs during anaphylactic shock and in certain other allergic manifestations.

THE HISTAMINE THEORY OF ANAPHYLAXIS

Generally speaking, the combination of antigen and antibody and its direct consequence, cell injury, are the basis or cause for an outbreak of any anaphylactic shock. If we can prove that not only is histamine liberated during cell injury following an antigen-antibody reaction, but that it is also *responsible* for the manifestation of shock, then the first junction of the three pillars of the triangle, histamine-anaphylaxis-antihistaminics, has been made.

Shortly after the completion of their pharmacologic study of histamine, Dale and Laidlaw were struck by the strong resemblance between the symptoms of histamine poisoning and those of the anaphylactic shock. As is well known, anaphylaxis, which was thoroughly studied by Richet after the turn of the century and first described by him in 1905, is one of the most unusual phenomena in animal pathology. Anaphylaxis may be defined as a potential state of reactivity produced in certain animals by sensitization against otherwise harmless substances. In short, the general procedure for producing anaphylaxis is the following: A guinea pig or dog receives an injection of 1 to 2 cc. of horse serum, which does not produce any unusual effects. If, however, three weeks later, a small amount of the same serum is again injected, the animal responds immediately with an acute, often lethal reaction. In guinea pigs, for instance, the anaphylactic shock starts with itching, asthma, followed by general convulsions and death, all within a few minutes. It has been established that, during the three-week incubation period, specific antibodies against the sensitizing agent or antigen have developed. These antibodies are sessile, fixed to certain cells of the so-called shock organ, and combine, as already stated, with the reinjected antigen. This combination takes place in or on the very cells where the antibodies are fixed, and initiates the course of an anaphylactic reaction.

Almost from the very beginning, it was supposed that a toxic substance, an anaphylactic poison, is liberated during this cell injury. This poison was supposed to be the direct and final cause of the anaphylactic shock. It is to Dale and Laidlaw's great credit that they were the first to associate this anaphylactic poison with histamine.

Several observations have led to the identification of histamine as the direct cause of anaphylactic shock. There is, first, the striking similarity of many symptoms of histamine poisoning and anaphylaxis. Secondly, there exists a very definite relationship between sensitivity to histamine and the tendency to become anaphylactic: the more an animal is sensitive to histamine, the easier it becomes sensitized. Finally, there is much and varied experimental proof pointing to the liberation of histamine during anaphylactic shock.

For a long time, on the basis of these first two facts, the indictment of histamine as the principal offender was an indirect one. No suitable methods were available which would permit the detection of very small quantities of histamine (amounts in the order of $\frac{1}{10,000,000}$ of a gram per ml.) as they occur in the blood and other fluids. Systematic experimental studies on the histamine metabolism in anaphylaxis were only started almost 20 years after Dale and Laidlaw's publication, when Gaddum and his co-workers, Code, Rose, and others had devised suitable methods. Today, the evidence pointing to the release of histamine in anaphylactic shock in most animals is overwhelming, for Gaddum and Barsoum, Code, Rose, Dragstedt, and many others could detect significant increases of serum histamine during and immediately after anaphylactic shock.

The histamine theory of anaphylaxis has been extended, lately, to include many of the naturally occurring allergic diseases. Previously, anaphylaxis was considered as a pathologic entity of its own, and this term was restricted to experimental animals.

Today, we know that anaphylaxis is only a part of a more general pathologic process, namely, of allergy. Anaphylaxis is considered as a special form of allergy which not only can be experimentally produced in dogs or guinea pigs by sensitization with proteins, but which sometimes occurs in man.

Furthermore, certain food allergies or so-called constitutional reactions occurring in man after injections of medicaments or after allergic skin tests show some of the typical symptoms we encounter in experimental anaphylaxis. Most authors agree today with the concept that anaphylaxis and the allergic diseases in man, such as food allergy, hay fever, pollen asthma, urticaria, and others are based upon the same mechanism. In all these cases, antibodies are formed which unite with the antigen, and in each instance it is the union of antigen and specific antibody followed by cell injury which produces the allergic symptoms. The principal difference between anaphylaxis and the various other forms of allergy is due to the location of cell injury and to the compositions of the injured cell.

We have much experimental data suggesting that histamine plays an important role in the symptomatology of many naturally occurring allergic diseases, as it does in experimental anaphylaxis. Increases in the histamine content of the blood have been found in cases of urticaria, hay fever, physical allergies, serum sickness, and others.

No doubt, the histamine theory offers a most appealing explanation for many phenomena occurring in anaphylaxis and other allergic manifestations. It satisfies our tendency for simplification. However, oversimplification distorts most facts. There are, indeed, many symptoms characteristic for anaphylactic shock, as well as many typically allergic manifestations, which cannot be explained by histamine release. Histamine, for instance, cannot be the cause for the loss of clotting ability of the blood during anaphylaxis in dogs. Furthermore, typical anaphylactic shock in rabbits, calves, and horses is not accomplished by a rise in the histamine content of the blood; in fact, there is a definite decrease. Certain authors therefore reject the histamine theory as a whole, or restrict it to certain symptoms. On the other hand, we must realize that the histamine theory does not necessarily postulate a rise in the histamine blood level.

Since it is known, today, that the antigen-antibody reaction takes place within the cells, we speak of the cellular theory of allergy, and no longer of the humoral. It is sufficient that histamine is liberated inside the cells, and it need not be released into the blood stream. However, it must be emphasized that the advocate of the histamine theory never intended to explain the entire process of allergy and all its symptoms only on the basis of histamine release. In addition to histamine, many other substances are liberated during cell injury, several of which are physiologically active, as, for instance, heparine, which is liberated after liver cell injury, various enzymes, glucose, etc., which in their turn may produce symptoms unlike those brought about by histamine. Histamine is only one, but nevertheless, in our opinion, the most important factor in anaphylactic shock in certain animals and in certain forms of human allergy.

New proofs, pro or con, are needed in regard to the histamine theory. A new proof for the correctness of the histamine theory of anaphylaxis and certain other allergic symptoms would arise from experiments with chemical substances which specifically counteract or neutralize the pharmacologic and physiologic effects of histamine

ANTIHISTAMINIC SUBSTANCES

Indeed, the following reasoning might have been made immediately upon the conception of the histamine theory of anaphylaxis. If the theory that the anaphylactic poison is identical with histamine is correct, then one would expect that a substance which nullifies the effect of histamine would also prevent anaphylaxis and all those symptoms of allergic diseases which are believed to be produced by a histamine release. Since experimental anaphylaxis and allergic diseases are so closely related, such an antihistamine substance would, therefore, not only confirm the histamine theory of allergy, but, at the same time, be the long-awaited remedy for hay fever, urticaria, and asthma.

However, this reasoning has only recently received its proper attention. In 1937, Bovet and Staub, of the Pasteur Institute of Paris, initiated a systematic search for substances capable of counteracting histamine. Among a great variety of compounds studied, they found two with high antihistaminic activity, namely, thymoxy-diethyl-ethylamine and diethylamino-ethylene-ethyl-aniline, the latter being three times more active. Both substances were able to nullify, in very small concentrations, the toxic effects of histamine on isolated guinea pig gut, lungs, etc., *in vitro,* as well as in the intact animal.

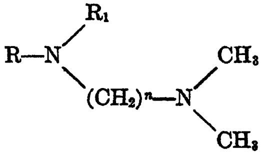
The French investigators then tried the two substances in anaphylaxis of guinea pigs and found that, in accordance with their prediction, both substances protected animals against shock. They concluded, from these results, that they had new and convincing proof supporting the histamine theory of anaphylaxis and, at the same time, an important contribution to anti-anaphylactic and, consequently, anti-allergic therapy. Unfortunately, both substances were so toxic for man that clinical use was impossible. These promising results led other workers to further their efforts to counteract histamine. They attempted to prevent anaphylaxis by the injection of histaminase, the histamine-destroying enzyme which we have already mentioned. This method was definitely worthless in practice, although there was some effect in experimental shock. Equally unsuccessful were the numerous experiments and clinical trials made to counteract histamine by the process of desensitizing the body against histamine either by injections of histamine itself, or of histamine-protein complexes.

Bovet and Staub's investigations were interrupted when war swept over Europe. In 1941, the antihistaminic project was again taken up in France, and many compounds analogous to Staub's derivatives were prepared by Mosnier. His substances were tested by Halpern, who, in 1942, reported on two new derivatives of Staub's compound 1571F which were not only much more active, but, at the same time, considerably less toxic than the earlier products. One of these substances, Antergan, was introduced into the clinic with great success, which marked, after epinephrine, the first significant step in the chemotherapy of allergic diseases. However, this remedy produced, in spite of its increased activity and tolerance, an untoward number of unpleasant side effects and was, moreover, ineffective in many patients. Further modifications were, therefore, attempted which led to the preparation and introduction of new derivatives: Neoantergan in France, Antistine in Switzerland, and Pyribenzamine in this country. At the same time, Rieveschl and Loew prepared and examined substances which were modifications of the very first active compound which Bovet had described in 1937 as a strong anti-histaminic, namely, the thymoxy derivative 929F. Benadryl is a member of this group.

Let us return, for a moment, to our three discussion points. We have seen that histamine is a normal constituent of the body which is present in an inactive form; that, under certain conditions, histamine is set free, producing serious toxic symptoms. One of these conditions is the cell injury following the union of antigen and antibody. It is, therefore, theorized that histamine is responsible for the symptoms of anaphylactic shock and many other allergic manifestations.

Now, we learn that substances have been found which not only are capable of counteracting histamine, but which also protect against anaphylactic shock. There remains, in order to close our triangle, proof that these substances act as antianaphylactics and anti-allergics only or principally because they are antihistaminics. We shall, therefore, in the remaining portion of this paper, discuss the mode of action of these antihistamine substances and follow their various activities *in vitro* and *in vivo*.

Since the antihistaminic substances so far investigated all act in the same way, we shall discuss, here, the activities of one member of this group, Pyribenzamine. The general formula of substances of this kind is the following:



All substances found thus far to be among the most active substances conform to this formula.

ANTIHISTAMINIC ACTIVITY

Antihistaminic activity *in vitro* has been tested by the usual methods, particularly by means of the guinea pig gut, guinea pig uterus, and isolated lungs and bronchi. Progressively more active compounds were found. Pyribenzamine shows an activity in concentrations as low as 0.02 per ml., while Halpern reports that a new substance, 3277 RP, a thiodiphenyl derivative, is, at least in this *in vitro* test, ten times more active. In the case of Pyribenzamine, it is evident that one molecule of Pyribenzamine neutralizes the action of 100 molecules of histamine. The antihistamine activity *in vivo,* when tested in guinea pigs, proves that there is a parallelism between the antihistaminic activity *in vitro* and that activity *in vivo.* Previous injection of 5 mg. of Pyribenzamine protects guinea pigs against 60 to 120 minimal lethal doses of histamine.

ANTI-ALLERGIC ACTIVITY

All these powerful antihistaminic substances protect guinea pigs from anaphylactic shock and, thus, become antianaphylactic remedies. If, with compounds within the same chemical group, we compare anti-anaphylactic activity with the antihistaminic activity, we find good parallelism. However, if we compare anti-anaphylactic and antihistaminic power of substances belonging to different groups, as, for instance, that of Pyribenzamine and Benadryl, the anti-anaphylactic activity is no longer directly proportional to the antihistaminic power. With Pyribenzamine, we obtain definite protection from anaphylactic shock in doses as small as 0.1 mg./kg.

CUTANEOUS SENSITIZATIONS

The clinical manifestations of allergy in man present the greatest variety of pictures, and there is no one organ which is not, in one allergy or another, affected in various ways and combinations. Until now, the allergic diseases experimentally reproduced in animals have been much less colorful, and restricted to certain types. Thus, it was possible regularly to produce in guinea pigs, by pretreatment with hog serum, generalized rashes which to some extent resemble general urticaria in food allergy in man, or generalized rashes in sensitizations to drugs. Pyribenzamine strongly counteracts this form of sensitization, thus proving that here, as in anaphylaxis, histamine plays a predominant role.

One of the most common allergic manifestations in man and in many animals is contact dermatitis, or eczema, and it is the general assumption that, contrary to urticaria, these diseases are based upon a special mechanism different from that responsible for urticaria or hay fever. It is likely that, here, substances other than histamine are involved. Indeed, Pyribenzamine and the other antihistaminic substances are least effective in these forms of allergies in man. However, in experimental sensitizations of dermatitic type, which we produced in guinea pigs by pretreating them with paraphenylenediamine, Pyribenzamine was quite active, although the doses required were very high. The effectiveness of Pyribenzamine was especially noticeable when Pyribenzamine ointment was locally applied. It seems that Pyribenzamine ointment is also effective in many cases of itching dermatoses in man.

Having seen, now, that these substances neutralize the activities of histamine *in vitro* and *in vivo*, there remains the question of explaining the mechanism of action. Histamine may be neutralized in different ways: The first method would be to prevent the formation of free histamine; or it should be possible to destroy it, as has been attempted by the introduction of histaminase; or, finally, there should be a means of preventing it from producing its physiological and pharmacological activities. This latter method is precisely what antihistaminic substances accomplish. It has been experimentally proved that they do not prevent the release of histamine, nor do they destroy released histamine. On the other hand, they do prevent histamine from exercising its typical activities. They supposedly prevent histamine from reaching those cells or enzymes to which histamine normally becomes anchored, and they are capable of occupying those places which histamine normally would occupy. In other words, antihistaminic substances compete with histamine for the normal histamine-receptive sites.

For this reason, no histamine effects appear. The same mechanism of action has been proposed for the neutralization of sulfonamides by PABA. And as PABA and sulfonamides are chemically related, so is there also a certain similarity between antihistaminic substances and histamine.

Since these antihistaminic substances do not influence the liberation of histamine but act only upon the last phase of the anaphylactic chain reaction, they do not constitute any etiologic therapy, but afford only symptomatic relief. Etiologic therapy would consist in using substances which prevent either the production of antibodies, the union of antigen and antibody, or the release of histamine from this union.

We have not discussed, here, the toxicity of the different antihistaminic compounds or the side effects occurring during their therapeutic use. Unfortunately, these are quite numerous. Therefore, throughout this country, intensive research has been started for the development of new products which will produce less side effects, and it is quite probable that our present substances will serve only as the important starting-point for a series of more effective remedies for allergic diseases.

In closing this discussion, I trust that I have been able to create interest in the complicated but very fascinating problems which arise when we speak of antihistaminic and anti-allergic substances.

SECTION OF PSYCHOLOGY

March 17, 1947

Doctor Kurt Goldstein, Visiting Professor of Psychology, College of the City of New York, New York, N. Y.: *Organismic Approach to the Problem of Motivation.*

"The problem of motivation," writes Boring, "is the problem of determining the forces which impel or incite all living organisms' actions."

It is not and cannot be my intention to take into reference the enormous research undertaken in this field, to discuss the different attempts to solve the problem, and to deal with it in all its implications. What I intend to do is to draw attention to some observations which seem to me suited to contribute something to our knowledge of these forces, and which support an organismic approach to the problem of motivation.

In all the cases I wish to refer to, we are facing the process of adjustment to an irreparable functional defect due to a damage of the brain. The way in which this adjustment is achieved gives us some insight into the motives of organismic behavior. These patients who show a number of defects—differing as to the location of the damage—must find adjustment to their functional defects. Otherwise, they are in danger of deteriorating increasingly. They are, thus, under the urge of a strong force like the normal organism if its activity is determined by a strong 'drive.'

The condition of such a patient during the first time after the incident occurred—it may be an injury, a hemorrhage, or any other disease which produces a more or less circumscribed damage to the brain—is best characterized as a state of *general disorder.* The patient is not only incapable of performing tasks he could perform before; he is, besides that, changed in his total behavior, he is restless, distracted, inconsistent, frightened, very emotional; he cannot fulfill tasks in which he is apparently not damaged. He is in a state which I have called 'catastrophic condition.'[1] After a certain time, his behavior changes. He shows greater constancy, is more quiet, no longer seems so often stricken by anxiety. The organism seems to have reached again a state of order, of equilibrium. What is the cause of the condition of disorder, and which are the forces that bring about the new order?

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[1] See, **Goldstein, K.** The Organism. A Holistic Approach to Biology: 35 ff. American Book Company. New York. 1939.

To answer these questions, we have to look at how the order and constancy of the normal organism are guaranteed. The basis of order is the constancy of the thresholds of the organism. If the thresholds would not remain approximately equal under 'normal' conditions (in the outer world and inside of the organism), a stimulus corresponding to an object or an event would not arouse about the same effect and the same experience, nor would the organism be able to react to the same situation in a consistent way. Our world would change incessantly, and we ourselves would change, too. Our personal existence would always be in danger. As a matter of fact, however, this is not the case. Our world remains relatively constant, despite all the changes in it, and so do we ourselves.

On the other hand, there is no doubt that each stimulation produces a change in the psycho-physical substrata by which its excitability is modified, with the result that a new stimulus, equal to the former one, gives rise to an effect different from the previous one—or, better said, would give, if there were not a mechanism at work by which that is prevented, by which the threshold is brought back to its previous, normal height after a definite period of time. This mechanism, which we call the *equalization process,*[2] is a basic biological phenomenon. It keeps the thresholds constant and is also the presupposition of ordered behavior, which guarantees the existence of the organism. This normal equalization process, in any part of the organism, is dependent upon the normal functioning of the total organism, as I was able to show on another occasion.[3] If a part of the organism is separated, 'isolated' from the rest, the equalization process is modified in this part. Stimulation here produces abnormal reactions, and ordered behavior is more or less impossible. This happens in pathology. The organism is no longer able to come to terms with the demands of his previous milieu. Disordered reactions, catastrophes, and anxiety are the consequences. That is the condition in which the patient is in the beginning.

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[2] Ibid.: 113 ff.
[3] Ibid.: 111.

How does he return to the state of order, if his impaired capacities do not come to the norm? As a matter of fact, he has the same defects in the state of order, but they no longer come to the fore in a disturbing way. That, however, is not the whole picture. If we compare the results of examination of the different performance capacities with the behavior of the patient in everyday life, we find an astounding discrepancy. Such comparison reveals, on the one hand, that the patient performs better in the latter condition (as we shall see by finding compensations for his defect); and, on the other hand, that he does not at all utilize all the possibilities of the preserved capacities. He seems to stick to some definite performances. This rigidity of behavior is usually attributed to a special anomaly called perservation. Closer investigation shows that there is no evidence to assume such a general anomaly; that the patient is not at all perseverating under all conditions; that he sticks to such performances as he can fulfill, and only if he is confronted with a task he cannot fulfill[4] does the perseveration occur. If examination is organized in a way that tasks of a kind which the patient cannot fulfill are avoided to a certain degree, he does not perseverate or only much less. Why does he perseverate if he is confronted with insoluble tasks? Because the insolubility of the task may produce a catastrophic condition, and with that make it impossible for the patient to use even his preserved capacities. Catastrophic condition does not only eliminate performance momentarily but has a disastrous effect on the activities following for a certain shorter or longer time. Thus, 'self-realization' of the organism and its 'existence' are in danger. That is what the organism tries to avoid by sticking to the performances it is able to fulfill, if confronted with such tasks as it cannot cope with. This prevents occurrence of catastrophic condition.

Now, the patient (as long as he is living in his old milieu) is easily and frequently confronted with tasks he cannot solve and, so, exposed to the entrance of catastrophe. This goes along with the experience of abnormal tension. It is only too natural that the sick organism tends to remove this dangerous condition by all means. Only then will it remain at least in a certain order. Thus, it seems to be governed by a 'drive' to eliminate tension, *to find release of tension,* which is at least temporarily achieved by sticking to a task it can master.

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[4] **Goldstein, K.** Physiological aspects of convalescence and rehabilitation, etc. Symp. Physiol. Aspects Convalescence etc., Fed. Proc. Exp. Biol. 3: 260. 1944.

All I have said about the behavior of organic patients is valid also for so-called neurotics. On the onset of the development of neurosis, disorder and anxiety stand in the center, as expressions of the impossibility to come to terms with tasks set for the individual, and of the experience of not being able to realize his or her own personality. This anxiety may be due to conflicts in infancy or later life. I cannot discuss this complex problem here. Such a discussion would bring us too far away from our subject and is not essential for the problem we are interested in. We are interested in the motives which bring about the elimination of disorder and anxiety in neurotics. The neurotic organism tries as well as the organic to get rid of disorder, by the same reason and by the same means. This it accomplishes by building up protective mechanisms which consist mainly in sticking to such performances which it can fulfill (in spite of the conflicts) and which prevent the conflicts from coming to the fore and, with that, unbearable tension. The result is the well-known compulsive behavior, which is not simply an expression of disease but mainly a protection against the danger of tension and of catastrophes. The significance of compulsive behavior shows up when one tries to force the patient to give up his compulsive actions. Then he comes into a state of enormous tension, anxiety, confusion, aggressiveness, *i.e.*, catastrophe. If he is allowed to stick to his compulsions, the patient is able to fulfill so many tasks that he can realize his personality at least to such a degree that life to him appears worth living. He appears forced to follow definite drives, trends, desires, and does so for the sake of getting rid of tension. His whole behavior seems to be dominated, motivated by getting release of tension, like that of the organic patient.

From such observations, one might be induced to consider the trend to come into a state of equilibrium, into order—the trend for release of tension—a basic motivating force of organismic life. This corresponds to a theory of motivation which has obtained widely spread appreciation. This theory considers the important, if not the essential motive of behavior elimination of tension, which appears as an expression of the various drives which are supposed to motivate organismic life. The goal of the drives, according to this theory, is the elimination of the disturbing tension which they produce. The best known expression of this tension-release theory is the pleasure principle of Freud.

I think we are not justified to make such a conclusion from the observation of patients, and that for the following reasons:

The domination of behavior by the tendency to release tension is a *pathological* phenomenon. It is an expression of defective functioning of the organism. As I was able to show in many examples, it is the effect of stimulation of parts of the organism isolated from the total organism. It is the effect of isolation, and the most characteristic consequence of pathology. The pathological organism is forced into these reactions, but they are not its goal. The real motive of the activities of the pathological organism is, like that of the normal, the trend to realize its capacities. It tries to realize the preserved capacities in the best possible way. The behavior of patients with brain damage, as that of neurotics, can be understood only from this point of view. It is a basic mistake of the tension-release theory of motivation that it tries to interpret normal phenomena from laws determining abnormal life *without taking into consideration the phenomenon of pathology.* This brings the theory to overlook that the trend for release of tension is in the foreground to such a degree because, for these individuals, it is the absolute presupposition for ordered behavior and, with that, for self-realization at least to a certain degree. Indeed, the tension-release theory is not based on pathological material alone, but also on observation of young children and animals under experimental conditions. The phenomena observed here are, however, also modified by the influence of isolation, which is here effective in a way similar to that in pathology. We have no time, here, to consider the effect of the 'fallacy of isolation' and its theoretical and practical implications. I wish to point to my discussion of this very important problem on another occasion and stress only a few points. The theory which considers a drive for maintenance or self-preservation as the basic drive of organismic life is due to the above-mentioned fallacy. Preservation is an expression of pathology, of decay.[5] The goal of the normal organism is not preservation of an existent state. The tendency of normal life is directed toward activity and progress.

The facts which seem to speak for the *existence of special drives* do not correspond to the natural behavior of the organism. They are *special reactions,* in special situations, of the total organism under special unusual conditions. If the tension corresponding to a special deficiency of the organism, *e.g.*, of nutrition or sex, etc., increases to a certain amount—because of abnormal conditions in the outerworld or within the organism—then the organism may be disturbed in general, in a way that catastrophe and, with that, impossibility to realize its nature are impending. Then, the deficiency must be eliminated by all means, and, under these circumstances, the organism appears motivated in its behavior by hunger, thirst, sex, etc. Under normal conditions, we are not forced to follow such needs even if they are strong. If we are involved in a very important activity, the neglect of which would bring us into danger, then we do not follow such needs. We give way to them if this is not in contrast to the activities of self-realization, or if it belongs to these. The impression of special drives appears when we consider the activities of the organism isolated from their relation to the total organism.

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[5] See, **Goldstein, K.** Human Nature in the Light of Psychopathology: 147. Harvard Un. Press. 1940; also, The Organism: 443.

This and similar assumptions are due to the neglect of the fact that definite reactions belong into a special context and cannot be transmitted to others—the *neglect of context,* as John Dewey has termed this phenomenon, which he, in an interesting paper about context and thoughts, calls "the greatest disaster which philosophical thinking can incur." By this neglect, through the fallacy of isolation, elements belonging to a special context become absolute by the fallacy of *universalization* and are often "converted into sweeping metaphysical doctrines." Indeed, consideration of the belonging of special phenomena to a definite context is not sufficient for understanding organismic behavior. It will prevent us from making too quick generalizations of observations made under certain conditions. But it does not render possible a decision as to whether a phenomenon belongs to the 'context' of the organism. For such a decision, the concept of 'self-realization' seems to present a possibility.

The traditional view assumes several drives which become effective according to the intensity of the individual drive. That is true. The intensity can, however, be evaluated only in relation to the total organism, by its value for the latter's self-realization under the given conditions.

Our standpoint has another valuable consequence. We are able to understand, from here, the *positive significance of tensions.* The organism is not only driven sometimes by the tendency to release tension, but it accepts tension *as a positive experience.* Normal behavior corresponds to a continual change of tension of such a kind that, over and again, that tension is reached which enables and impels the organism to further activities according to its nature.[6]

The *tension-release theory* (as, *e.g.*, in the form of Freud's) knows only the *urge to release,* not the *pleasure of tension,* which is not only at the basis of all creative activity, but also an essential part of the pleasure corresponding to all so-called lower needs, as hunger, sex, etc.

Certainly, I do not want to deny that there are also normal conditions in which the need for rest and release of tension is in the foreground. Also, normal life is full of catastrophes, and we can fulfill our goals only if we have intercessions of rest and release of tension. But how much we submit to these needs, is determined by their significance for self-realization of the total organism. If they come to the fore as *all-determining* forces, then there is something wrong with the total organism, with the personality.

It may seem that I have drawn on the pathological observations mainly from the standpoint of warning against too simple an application, to the interpretation of normal behavior, of the laws found valid there. However, I also stressed a positive factor as a motivating force which, admittedly, did not come to the fore so much in the first part of this paper, though it already showed its great significance, *viz.*, the trend of the sick organism to utilize its preserved capacities as much as possible. To stress this factor is the purpose of my further discussions, which will use the procedure in adjustment to a circumscribed irreparable defect as a basis for determining the motivating force.

Because the organism must be very careful concerning the occurrence of catastrophes, it has to restrict itself in the utilization of all its preserved performances: a selection has to take place. This selection becomes understandable if one considers it from the angle of inquiring by which performances the goal, self-realization, can best be reached and catastrophes may be avoided at the same time.

I should like to illustrate this assertion by two examples, the behavior of patients with hemianopsia and with disturbances of equilibrium due to cerebellar lesions.[7] Hemianopsia is total blindness of corresponding halves of the visual field in both eyes due to lesion of the occipital lobe of the brain. Behavior of these patients in everyday life often fails to indicate that they do not see in one half of the field. They are subjectively aware of a somewhat impaired vision, but they see objects in their entirety, not only halves of them. Their visual field is arranged around a center like in normal people, the region of clearest vision lying *within* this field—not, as one could assume, on the margin corresponding to the location of the fovea in the preserved half of the field. I cannot explain, in detail, how the organism manages to obtain a whole field. I wish to stress the fact that research has shown that only if the object one wants to see is within a field good vision and recognition are guaranteed. This has to be reached by the patient, and it is achieved by a little deviation of the eyes, so that the visual stimuli coming from all sides are then reflected on the healthy retina. This is the only way to enable the individual to have visual performances which make recognition of objects possible, and it apparently occurs. Only from this point of view the new organization becomes intelligible. It is apparently *the goal of the organism* to have a vision as good as possible, and that was the motive of the activity.

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[6] The Organism: 197.

[7] As to the facts, see, **Goldstein, K.** The two ways of adjustment of the organism to cerebral defects. J. Mount Sinai Hosp. 9(4): 504. 1942; also, The Organism: 47 ff.

This assumption is confirmed by another observation. The transformation mentioned does not take place if the one half of the visual field is not *totally* lacking perception, as in cases of hemiamblyopia, where vision in the one half is diminished but not so much that it could not be used for essential performances and, particularly, for rendering possible the normal, characteristically formed, visual field.

As long as such is the case, transformation does not occur, and it does not do so because it is not necessary, since the diminution in vision in hemiamblyopia is not so disturbing that recognition of objects cannot take place. This state has some advantage over that of transformation. The latter limits other performances of the organism. After transformation, the field is constricted, and by displacement of the eyes the total organism is restricted in some performances. The organism bears all these impediments if a good vision—a performance so essential for the total organism—cannot otherwise be reached, as in total blindness of the halves of the visual field. It avoids them if adequate vision can be maintained to some degree, sufficient for the most important performances for the total organism: *recognition of objects,* without eye-shifting with its consequences. Apparently, what is pertinent is not the best possible performance in *one* special performance-field but the best possible activity of the organism as a whole. Here lies the motivation for the particular form of adaptation, the cause of the differences of adaptation in hemianopsia and hemiamblyopia.

Another example from a totally different field will illustrate the same procedure of the organism in adjustment to a circumscribed defect.

In some patients with cerebellar lesion, one observes tilting of the body and the head toward the side of the lesion—in left-sided lesions to the left side. In other cases, however, with a similar lesion, the head is tilted to the other, the right side.

What is the cause of the deviation and of the difference in some patients with the same lesion of the cerebellum?

Closer investigation shows that the patient can stand and behave quite normally in all other performances, bodily and mentally, if he has head and body a little tilted to the left side. If he is forced to bring his head into the previous normal, erect position, he is not only in danger of falling to this side, but he feels dizzy, confused, and cannot fulfill other physical and mental performances in a normal way. He feels well and returns to the norm in all these respects if he is allowed to keep his head tilted sideways.

If he keeps his head straight, he experiences a pull to the affected side. This pull corresponds to a normal tendency of the organism *to turn toward stimulation* from the outside, stimulation of the skin, ear, eye, etc. The organism is thrown to the side of stimulation. The normal individual does not feel this pull because both sides of the body are normally stimulated in about the same strength and, so, the pulls to both sides counterbalance each other. Thus, an equilibrium is established which is further guaranteed by the regulative influence of the cerebellum and frontal lobes, and we stand up straight without difficulty. In the patient, the reaction is abnormally strong on one side because of the lesion of the cerebellum of the corresponding side. The organism is thus thrown to this side by the stimuli originating from the normal outerworld. If the patient tilts his head, the pull disappears. The abnormal position represents an adjustment of the organism to the defect, and brings about a new order. The organism is able effectively to utilize its capacities to a higher degree because of the lack of catastrophes. The patient finds by, so to say, yielding to the abnormality, to the abnormal pull, a better, more constant condition.

The deviation becomes so understandable from the motive that the organism tries to get a condition in which it is in a *situation to realize its capacities best.*

Now, why does the other type of patient, suffering from the same defect, hold the head *tilted to the opposite side?*

If one examines these patients carefully, one finds that they have the same defect, but in a *different degree.* The pull has the *same direction* but is *much stronger.* In the first group of cases, the patient shows a *tendency* to fall if he keeps his head upright, in the second the patient *falls really,* his stability thus being affected to a much higher degree, and he is more easily thrown into great disorder with disturbance of all performances. If this patient followed, in his attempt to adjust to the defect, the same course as the first one, *i.e.,* if he yielded to the pull, he would have to assume an extreme position in his attempt to compensate the stronger pull by tilting the body. By this position of the whole body, no balance could be maintained at all. Such an attempt of adjustment to the defect would not bring order, and, thus, the behavior of the organism as a whole would not be guaranteed. Therefore, another way of adjustment has to take place, namely, the pull to the left side is compensated by the production of a voluntary pull to the other, the right side. This is achieved by tilting the head to the other side. By this procedure, equilibrium and a new order are obtained, which is confirmed by the fact that, under this condition, the patient feels best and shows normal behavior in all performances.

The difference of adjustment to the same, only quantitatively different defect, again shows that the *motive is to find a new order for the total organism.* That form of adjustment occurs which best guarantees this order under the given condition. We have, as in hemianopsia and hemiamblyopia, here, too, two different, yet *opposite* ways of procedure motivated by *the same trend,* on the one hand yielding to the defect, on the other hand *transformation of the whole function* of the concerned apparatus. Both ways do not represent adjustment of equal value for the total organism. Yielding is more natural, more automatical, demands less voluntary activity, and therefore insures more security. In the second case, the *normal form of functioning is changed.* It is more voluntary, leads more readily to fluctuation of function, includes less security, and admits greater possibilities for catastrophic reactions. But it is the only way to guarantee the order of the whole organism *under the given condition.* Therefore, it is chosen, but only if the other way is impossible. Again, we see as a determining factor the tendency to achieve the optimal performance of the total organism.

It is certainly of general interest that the *same motive* can bring about *different, yet opposite, effects in behavior*—a phenomenon which we meet with all motivating factors. A strong need can be fulfilled by yielding or opposition, and that procedure is selected which best guarantees the possibility of self-realization of the *total* organism.

We have considered, till now, the way of adjustment from the viewpoint of the condition in a special part of the organism (a defect in a special performance field). However, the condition of the rest of the organism and the demands which arise from the outerworld have to be taken into consideration as well if we want to understand the behavior of the patient. Adjustment to a special defect is totally different if the special capacity could no longer be used by the organism because it has other defects which would make this impossible. For example, a patient with visual agnosia, *i.e.,* defect of recognition of seen objects, will try to improve his defect in such recognition by building up non-visual performances as substitutes. But he will do it *only if he can use them.* A patient who cannot use them because he is paralyzed at the same time and, thus, hampered in activity in general, will not build such substitutes at all or only to a smaller degree.

A corresponding procedure takes place if the demands coming from the outerworld differ in degree. If they are too great in respect to the patient's capacities, he gives in and does not adjust himself to the highest possible degree, apparently because it would be in vain and any attempt would produce catastrophe. He satisfies himself with a lower form of adjustment and, if he is not allowed to do that, he breaks down. If the demands are too small and the patient can do justice to them without changes of behavior, these do not occur. It again becomes evident that the motive of the way of adjustment is determined by the trend to come into a state which makes possible the best way of self-realization under the condition of the irreparable defect.

To summarize: *What can we gain from the experiences with patients with brain damage for our knowledge concerning the forces which determine motivation?* We saw that adjustment is determined by the significance which a special activity has for the organism as a whole, and that the motivational force of this activity—yielding or transformation of function—depends upon this significance.

This is valid for adjustment to an irreparable defect as well as for the effect of normal needs. Any explanation is doomed to failure which attempts to reduce human motivation to a number of *isolable factors,* capacities, drives, etc., to *separated* forces which interact only secondarily with each other. Our analysis has shown that such constant and independent forces may determine behavior *under pathological and under unnatural, eventually emergency conditions.* They are due to the influence of isolation on the behavior which is characteristic for all these conditions. We have no right to infer, from these phenomena, the existence of such single forces in the normal organism.

Pathology teaches us that we have to be cautious with all such inferences. Thus, for instance, we have no right to assume, from these experiences, that the clue for motivation normally is tension-release. Such an assumption neglects the factor of pathology, of isolation in general, in modifying behavior: it is the effect of the fallacy of isolation.

Our main result was that the basic trend of the organism is the trend *to realize its capacities,* its *personality* as much as possible, that it is the trend for *self-realization.* Forces which may gain influence, as special needs, can be evaluated in their motivational effect only if they are considered from this point of view, from *their significance for self-realization of this or that organism*—normal and pathological under this or that condition.

Here, we have a new starting point and task for research. Our approach traces the whole problem of motivation back to the problem of the organization of the organism, its capacities and potentialities, *i.e.,* its *nature.* We are confronted, indeed, with a difficult scientific problem of methodology, namely, how to gain knowledge about this nature of the organism from phenomena which we have to obtain by an analytic, isolating procedure of investigation. Especially, the question has to be answered of how to differentiate between the phenomena that are more or less essential for the organism and such that are due to the artificial isolation. Certainly, I cannot enter into the discussion of this basic problem of psychology and anthropology here. I again wish to refer, in this respect, to my book, *The Organism.* There, I have discussed the general methodological problem involved and tried to find rules for this differentiation by pointing to the *material* gathered under the concept of *preferred behavior*. This material seems to hold promise to make our task possible, at least in so far that we learn some steps of our way to the goal. The problem of motivation will not come on a more rational basis until the functional structure of the organism is better understood. I think that, in this respect, pathology can contribute very much, indeed, only if the phenomenon of pathology itself is taken into correct consideration.

I am fully aware that my dealing with the problem of motivation was incomplete, which is due partially to the complexity of the problem which could scarcely be disentangled sufficiently in one lecture. I was able to treat the problem only in a somewhat sketchy way. I could not do much more than to point to some material—by the way, easily to be multiplied—which may be useful in further discussion, and give some hints as to procedure in the attempt to find a more factual and reasonable basis for a theory of motivation. What I regret particularly is that I had no time to expose the implications of my interpretation of the facts as regards therapy, in patients with organic diseases as well as with neuroses. My point of view grew from the attempt to help patients. The problem of motivation is closely related to therapy. A theory of motivation should never forget that we are not primarily interested in this problem for the sake of knowledge, but for the sake of helping, guiding, of trying to change human behavior for the better. Therapy, on the other hand, cannot do anything more sensible than to observe the motivational tendencies operative in nature in the attempt of adjustment to defects, and then to try to follow these tendencies. There could be, in turn, no better proof for the soundness of this organismic approach to motivation than its usefulness for the purposes of therapy.

SECTION OF ANTHROPOLOGY

March 24, 1947

Doctor Julian H. Steward, Department of Anthropology, Columbia University, New York, N. Y.: *American Culture History in the Light of South America.*

The data of the *Handbook of South American Indians* suggest a four-fold classification of native South American cultures: (1) the Marginal, or hunting-and-gathering tribes; (2) the Tropical Forest peoples; (3) the Circum-Caribbean tribes; and (4) the Central Andean civilizations.

The Marginal tribes resemble the non-farming tribes of North America in their lack of certain advanced technologies, such as farming, loom weaving, woven baskets, dugout canoes, building arts, and metallurgy, and in the presence of innumerable items found mainly outside the area of farming, such as twined or coiled baskets, bark canoes, scratching sticks, drinking tubes, hockey and other games, etc. These may be survivals of a pre-agricultural, inter-American culture, but Marginal sociopolitical groups show evidence of local development, with strong environmental conditioning.

A basic civilization built on agriculture became established in the highlands, from Bolivia to Mexico, and in Yucatán. It had intensive farming, developed weaving, ceramics, metallurgy, and building arts, a class-structured society, incipient states, and a priest-temple-idol cult. These generalized features are an abstraction of a culture which had distinctive local forms but which, in its inter-American aspects, may be called the Formative Period culture.

From the Formative Period culture developed the distinctive Andean patterns, noted for their technological attainments and for their political developments.

A Formative Period type culture spread, with certain distinctive South American features, to the Maya frontier in Central America, beyond which it had Mexican characteristics, and across northern South America to the Antillean Arawak. Its principle features were: matrilineal decent, classes based particularly on priestly and warrior groups, a priest-temple-idol cult, and such rain forest adaptations, shared also with the Tropical Forest peoples, as cultivation of fruits and root crops, dugout canoes, pole-and-thatch houses, hammocks, bark cloth, and blow guns. The general pattern and certain specific elements of the Circum-Caribbean culture are also found in the southeastern United States, though the latter area is likewise linked to Mexico in some features.

It is postulated that the Tropical Forest tribes of Venezuela and the Guianas borrowed farming, pole-and-thatch houses, dugout canoes, pottery, and loom weaving from the Circum-Caribbean peoples, but they did not borrow the class-structured society or the priest-temple-idol cult. This material culture spread up the Amazon, but faded at its headwaters, leaving the tribes in a Marginal position culturally. These tribes are distributed in a great "U" around the Amazon Basin, and they extend to eastern Brazil and south through Mato Grosso, the Gran Chaco, and Pampas to the Chilean archipelago. The Tropical Forest technology was also spread, principally by Tupian tribes, down the coast of Brazil in the rain forest areas, and across Paraguay even to the Andes.

SECTION OF PHYSICS AND CHEMISTRY

MARCH 25, 1947

MR. J. J. BIKERMAN, Merck and Company, Inc., Rahway, N. J.: *Tackiness and Adhesion.* (This address was illustrated by lantern slides.)

(This paper will be published in the May issue of TRANSACTIONS.)

MR. ALLEN S. KENYON, Department of Chemistry, Columbia University, New York, N. Y.: *Higher Order Tyndall Spectra in Monodispersed Sulfur Sols.** (This lecture was illustrated by lantern slides and a demonstration.)

When light is passed through a relatively monodispersed colloid, in which the radius of the particles is roughly comparable to the wavelength of light and the index of refraction of the medium and dispersed phase differ appreciably, beautiful optical effects will be observed. These optical effects can appropriately be designated, "Higher Order Tyndall Spectra."

If white incident light is used, red and green bands will appear at various angles. The number of distinct bands of one wavelength that are observed in 180° will be set equal to the number of orders. This number does not necessarily correspond to any theoretical basis. The blue Tyndall spectra in the Rayleigh region ($r \ll 1/10$ the wavelength of light) will be assigned the order zero.

The sulfur sol system has been used for a quantitative study of these optical effects because it is ideally suited, since sulfur particles are spherical and are colorless to visible light. Sulfur sols can also be prepared relatively monodispersed by the action of acid on sodium thiosulfate, provided that the concentration range is .0005 to .0015 molar with respect to the thiosulfate, and if the acid does not exceed .003 molar.

The formation of sulfur particles from the acid-thiosulfate can be divided into three phases, namely: (a) Homogeneous, (b) Particle Transition, and (c) Heterogeneous. The homogeneous reaction begins immediately upon mixing the acid and thiosulfate with the formation of molecularly dispersed sulfur and proceeds until a definite degree of supersaturation is reached. At this point, sulfur particles begin to form, as evidenced by the appearance of the blue Tyndall beam. After the transition, the particles continue to grow. These three phases have been followed by means of transmittance at a wavelength where only the increase in sulfur would change the optical density of the system. Regardless of the initial concentration of thiosulfate and acid used, the optical density at the point of transition remains constant. This means that reaction continues to be homo-

* This address was delivered at the meeting of the Section of Physics and Chemistry on February 28, 1947.

geneous until a definite degree of supersaturation is reached. It was found that the concentration of molecularly dispersed sulfur present at the time of transition was 5×10^{-6} gram atoms per liter. The formation of sulfur particles is an agreement with Von Weimarn's conclusions on "corresponding crystallization conditions." The kinetics of this reaction are being thoroughly investigated by Miss Ethel Zaiser under Prof. LaMer's direction.

The sulfur sols produced are more monodispersed than other sulfur sols that have been studied in the past. These sols have been examined under the ultramicroscope and the electron microscope. However, to date, no distribution values are available. Qualitatively, these sols are relatively monodispersed.

The angles at which orders appear are functions of the composition of the incident light, the particle size, and the relative refractive index of the particle to that of the medium. When the sulfur particles reach a size to produce orders, that is, $\alpha = 2$ where $\alpha = \frac{2\pi r}{\lambda'}$, r being the radius of the particle and λ' the wavelength of light in the medium, the order begins to form in the forward direction. As the particles grow, the number of orders increases from one to nine. This is the maximum number of orders that have been clearly distinguished.

The angular positions of orders are calculated theoretically as a function of size for a particular relative index of refraction. The calculations were based upon the Mie theory and were made for particle size studies on aerosols during the past war. In order to measure the particle size, it is necessary to measure the actual angular position of orders of a particular wavelength and read the size directly from the theoretical plot. The theoretical calculations were checked by an independent method using Stokes' Law.

The transmitted light can also be used as a means of determining the particle size. Moreover, this enables the number of particles per cc. to be measured. The total decrease in transmittance is due to scattering and any energy lost by molecular absorption. The optical density is expressed by

$$\log Io/I = K_s \pi r^2 n l/2.3, \text{ or} \tag{1}$$

$$\log Io/I = K_e \pi r^2 n l/2.3 \tag{2}$$

where Io/I is the ratio of the incident to transmitted light, K_s the scattering area coefficient, K_e the extinction area coefficient, n the number of particles per cc. and r the radius of the particles. EQUATION 1 applies to wavelengths where there is scattering only, and EQUATION 2 to those wavelengths where absorption is also present. K_s and K_e have units of area and are equal to the optical area effective in scattering light.

A plot of the K_s or K_e values against α for a constant relative refractive index passes through a minimum. When the transmittance of a sol is measured as a function of wavelength, the density will pass through a minimum which also corresponds to the theoretical minimum. The radius of the particles is then calculated from the relation $\alpha(\text{min.}) = \frac{2\pi r}{\lambda'(\text{min.})}$. The number of particles is calculated by using EQUATIONS 1 or 2, depending on whether or not absorption is present.

If the particles absorb, then the relative index of refraction becomes complex and is equal to $m = m'(l\text{-}ik)$, where m is the complex index, m' the ordinary index, and k the absorption index. This introduces a complication in the transmittance method, and the case is being thoroughly studied in the ultraviolet, where sulfur particles absorb. The details of this investigation will be reported elsewhere.

It has been shown how two different optical methods can be used to determine size when the particle size is comparable to the wavelength of light. Also, the transmittance method gives a simple means of determining particle number. These methods are only applicable for a limited number of systems, because of the very limited theoretical calculations available at present. As soon as the calculations are extended, these methods will be adaptable to many systems.

I wish to acknowledge due credit to Prof. V. K. LaMer and his other research students who made this presentation possible through their work.

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GENERAL CONFERENCE

MARCH 21 and 22, 1947

Conference on *"The Adrenal Cortex."*

The New York Academy of Sciences and The New York Academy of Medicine jointly held a Conference on "The Adrenal Cortex." Doctor Robert Gaunt, Syracuse University, Syracuse, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

FRIDAY, MARCH 21

Morning Session. "Introductory Remarks," by the Conference Chairman.

"The Pituitary-Adrenal System," by George Sayers and Marion A. Sayers, Department of Pharmacology, University of Utah School of Medicine, Salt Lake City, Utah.

"The Chemistry and Partial Synthesis of Adrenal Steroids," by Edward C. Kendall, Division of Biochemistry, Mayo Foundation, Rochester, Minnesota.

"The Bioassay of Adrenal Cortical Hormones," by Ralph I. Dorfman, Department of Medicine and Biochemistry, Western Reserve University, School of Medicine and Lakeside Hospital, Cleveland, Ohio.

Afternoon Session. "Some Studies on the Role of the Adrenal Cortex in Organic Metabolism," by Dwight J. Ingle, Research Laboratories, The Upjohn Company, Kalamazoo, Michigan.

"The Cytology and Cytochemistry of the Adrenal Cortex," by Roy O. Greep and Helen Wendler Deane, Harvard School of Dental Medicine, and Department of Anatomy, Harvard Medical School, Boston, Massachusetts.

"The Adrenal Cortex and Its Tumors," by George W. Woolley, Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine.

SATURDAY, MARCH 22, 1947

Morning Session. "Urinary Excretion of Adrenal Cortical Steroids," by Eleanor H. Venning and J. S. L. Browne, Royal Victoria Hospital, Montreal, Canada.

"Adrenal Cortex Function in Stress," by Gregory Pincus, Worcester Foundation for Experimental Biology, Shrewsbury, Massachusetts.

"The Diseases of Adaptation," by Hans Selye, University of Montreal, Montreal, Canada.

Afternoon Session. "Clinical Studies in Addison's Disease," by George W. Thorn, Peter H. Forsham, F. T. Garnet Prunty, and A. Gorman Hills, Department of Medicine, Harvard Medical School, and the Medical Clinic, Peter Bent Brigham Hospital, Boston, Massachusetts.

"Cushing's Syndrome—A Disease of the Adrenal Cortices?" by E. J. Kepler, Mayo Clinic, Rochester, Minnesota.

NEW MEMBERS

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SER. II, Vol. 9 MAY, 1947 No. 7

SECTION OF GEOLOGY AND MINERALOGY

APRIL 7, 1947

DOCTOR H. H. HESS, Associate Professor of Geology, Princeton University, Princeton, N. J.: *Major Structural Trends of the Western North Pacific.* (This lecture was illustrated by lantern slides.)

During the war, a huge number of soundings were obtained by the United States Navy in the Pacific. Many of these data were collected under combat conditions, and many areas were traversed which would not be crossed by ships in peace-time. The data obtained are being compiled at the Hydrographic Office. The first result of this compilation is the new bathymetric chart, *Korea to New Guinea*, including the Philippine Sea (H.O. 5485).*

Within the region of the above chart, the world's most rugged topography is found. There is a difference of more than 40,000 feet between the mountains of the eastern Philippines and the bottom of the great Mindanao Trench. Other great trenches of the circum-Pacific belt lie to the east of Honshu, the Bonins, the Marianas, Yap, and the Palaus. These form a fairly continuous chain and may be extended northward *via* the Kurile and Aleutian Trenches to North America, so that they form a series more than 6,000 miles long. To the west and north of these trenches are found the island arcs and areas characterized

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* A manuscript dealing with the area from Korea to New Guinea will be published in Bull. Geol. Soc. Am. For a paper dealing with some peculiar flat-topped peaks in the Pacific Basin, see in Am. J. Sci. **244**: 772–791. 1946.

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by andesitic volcanism, while to the south and east lies the gentle topography of the Pacific Basin, an area of basaltic volcanism.

The *Korea to New Guinea* area is probably also the most highly seismic region in the world. Within it, we find extremes of topography, earthquake activity, volcanism, and also gravity anomalies. This wealth of activity and the large scale of the structures makes it an ideal place to study large tectonic features and their causes. The deep trenches are the topographic expression of the dominant structural feature present. All of the other large structural features are related and subordinate to the crustal buckle beneath the trenches. It is thought that the buckling is related to convection currents in the substratum.

The region from New Guinea to New Zealand is in many respects similar to that discussed above. Here, also, there are extremes of topography, seismic activity, and volcanic activity. This region presents some further problems not seen in the former area. Arcs convex towards the continental area (Australia) are found in the New Hebrides, Solomons, and New Britain, which are the only exception to the rule in present-day arcs. The southern end of the westward-facing arcs is joined to the northern end of an eastward facing arc probably by a great zone of shearing and transcurrent faulting which extends from south of the New Hebrides to the northern end of the Tonga arc.

The Pacific Basin proper has a fairly level, gently undulating floor from which rise abruptly hundreds of peaks which form the present atolls, volcanic islands, and submerged ancient islands, the guyots (described elsewhere). Besides these, there are some very large gentle swells, the forms and distribution of which are as yet very imperfectly known. A belt across the North Pacific from the Equator to 30° N is fairly well explored and the data await compilation. The South Pacific basin remains largely unexplored. Through it passes a very large swell, the so-called Albatross Plateau.

SECTION OF BIOLOGY

April 14, 1947

Doctor Yale Kneeland, Jr., Department of Medicine, College of Physicians and Surgeons, Columbia University, New York, N. Y.: *The Common Cold.* (This lecture was illustrated by lantern slides).

(No abstract received.)

DOCTOR RAYMOND L. LIBBY, Stamford Research Laboratories, American Cyanamid Company, Stamford, Connecticut: *The Use of Tagged Antigens in Immunochemical Studies.** (This lecture was illustrated by lantern slides.)

There are a number of problems to be considered before embarking on a program of biological research involving radio-tracers. The first is what radioactive isotope to use. For biological work, the most desirable radio isotopes would be oxygen, nitrogen, carbon, and hydrogen. Of these four elements, there are no suitable radio isotopes available for oxygen or nitrogen. Carbon has two isotopes, C^{11}, with a half-life of 21 minutes, and C^{14} with a half-life of over 1,000 years. Hydrogen has one radioactive isotope, H^3, with a half-life of 20 years. There are a number of difficulties in using these three isotopes: C^{11}, because of its short half-life, is limited in its usefulness, while C^{14} and H^3 are limited in their usefulness because of the difficulties in measuring their soft radiation.

Among the other elements that might be used in biological studies, there are a number of very useful radioactive isotopes, such as P^{32} with a half-life of 14.3 days, S^{35} with a half-life of 88 days, I^{131} with a half-life of 8 days, Ca^{45} with a half-life of 180 days, Fe^{59} with a half-life of 47 days, and Na^{24} with a half-life of 14.8 hours.

A second problem to be considered is the synthesis of the compound to be studied. In our case, we are interested in tagged antigens or proteins, and, since these cannot be synthesized directly in a test tube, we are forced to adopt indirect methods through the mediation of the living cell.

A number of workers[1–4] have demonstrated that the nucleic-acid portion of the nucleoprotein molecule can be tagged by injecting or feeding animals with sodium phosphate in which the phosphorus is radioactive. Bacterial proteins containing phosphorus may be tagged by growing the organisms in a nutrient solution containing $Na_2HP^*O_4$. The method we have used to tag the tobacco mosaic virus (TMV) molecule will be given in detail subsequently. There are a number of ways in which proteins may be tagged directly. For example, radioactive Arsenilic-azo protein could be prepared by using radioactive arsenic, or radioactive iodinated proteins may be prepared by using radioactive iodine.

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* This address was delivered before the meeting of the Section of Biology on February 10, 1947.

The third problem that must be considered is the specific activity. This refers to the ratio of the radioactive atoms to stable atoms in the compound. In most biological work, it is desirable that the specific activity be relatively high in order to permit one to follow the compound through several half-lives and to overcome the relatively large dilution factors involved in most of this work.

The fourth problem to be considered is that of exchange between stable elements and radioactive elements. It is impossible to interpret the data obtained in certain problems unless the possibilities of exchange are known. For example, if one were to study the distribution of radioactive iodoglobulin in the body, it would be essential to know the possibilities of exchange between the tagged iodoglobulin and sodium iodide. *Vice versa,* one should know the possibilities of exchange between radioactive sodium iodide and untagged iodoglobulin.

The work to be reported on was undertaken in an effort to explore the usefulness of radio tracers in determining the distribution and the rate of breakdown of injected antigens. More specifically, we set out to try to answer the question of whether or not antigen must be present for the formation of antibody. Among the various theories of antibody formation, two are especially notable here. One postulates that antigen acts as a template and thus must be present for the formation of antibody, while the second postulates that the antigen acts to modify some enzyme, which in turn is involved in the synthesis of antibody globulin, and that even after the antigen is broken down more antibody may be synthesized.

For the present study, it was decided to attempt to tag tobacco mosaic virus. This virus was chosen for a number of reasons. First, it was a nucleo-protein and thus contained phosphorus; second, it could be obtained in reasonable quantities by extraction from infected tobacco plants; third, methods were available which would permit the preparation of highly purified solutions; and, fourth, tobacco mosaic virus had been reported in the literature to be an excellent antigen.

The TMV was tagged by inoculating seedling Turkish tobacco plants with the virus. These plants were then transferred to individual bottles containing a nutrient solution composed of KNO_3, $MgSO_4$, $CaCl_2$, and the necessary trace elements. Sodium phosphate containing 278 microcuries of radio P was then added to each bottle. The specific activity of the phosphorus in the $Na_2HP^*O_4$ was 70 microcuries per mg. of phosphorus. The level of the radio activity in the nutrient solution was maintained by weekly additions of $Na_2HP^*O_4$ or a complete change of the nutrient solution.

After thirty days, the stems and leaves of the tobacco plants were removed, macerated, and the TMV extracted and recrystallized three times. It was found that this purified preparation contained considerable radioactivity. From the radioactivity found per mg. of TMV and the reported percentage of phosphorus in TMV, the specific activity was calculated to be 7.4 microcuries per mg. of phosphorus. This is only one-tenth of the specific activity in the $Na_2HP^*O_4$ used. This dilution can be accounted for by the presence of inactive phosphorus in the original seedling tobacco plants. That the radioactivity was associated with the virus molecule, was demonstrated by the fact that the specific activity remained constant through several recrystallizations.

Each of 105 mice were injected intravenously with 0.4 ml. of the crystalline radioactive TMV. This volume contained about 0.6 mg. of virus with 3600 counts per minute. Each of these 105 mice, plus 5 controls, had been given an initial injection of normal TMV 18 days previously. This procedure was followed because we wanted to be sure of having readily measurable amounts of circulating antibody after injecting the radioactive TMV. For the duration of the experiment, the mice were kept in metabolism cages, so that the feces and urine could be collected at various intervals for the determination of the amount of radioactivity excreted.

Twenty-four hours after the injection of the tagged virus and at intervals up to thirty days, groups of 5 mice were etherized and bled from the heart and the serum samples pooled for the titration of circulating anti-TMV. After bleeding the 5 mice, approximately 250 mg. slices of liver were removed from each mouse and the samples pooled. These were placed in a metal cup and dried at 125° C. for twenty-four hours. The dried samples were then weighed and put in a furnace and ashed at 600° C. for twenty-four hours. The ashed samples were used for the determination of the radioactivity. It was felt that ashing was necessary because of the variable amount of self-absorption observed when counts were made on the dried material. The whole spleens from the 5 mice and aliquots of the feces and urine, and in some cases the remainder of the carcass, were dried, weighed, and ashed in a manner similar to that described for the liver.

The radioactivity of the ashed samples of liver, spleen, feces, urine, and carcass was measured by means of a Geiger-Müller counter and a mica window counter tube. The counting was done under rigidly standardized conditions, so that the day-to-day measurement of the radioactivity would be comparable. The radioactivity found in each tissue was expressed as counts per minute per milligram of tissue and was corrected for decay and self-absorption wherever necessary.

Autoradiographs were made of sagittal sections of mice, 24 hours after the intravenous injection of radioactive $Na_2HP^*O_4$ and tagged TMV, respectively. These autoradiographs were prepared by freezing a killed mouse in a dry ice acetone bath and then cutting a 3-4 mm. sagittal section with a band saw. The sections were then lyophilized and impregnated with paraffin, and 1 mm. sections were prepared from them with a microtome. The 1 mm. sections were placed in intimate contact with x-ray film and exposed for varying periods to obtain the autoradiographs.

Twenty-four hours after the injection of radio $Na_2HP^*O_4$, there was a very general distribution of radioactivity, so that the outline of the whole section was readily visible as well as cross-sections of various tissues such as the liver and the intestines. However, the greatest concentration of radioactivity was found in the bones as shown by the tooth, ribs, backbone, and pelvic bones. An area just under the intestines showing marked radioactivity was tumorous tissue.

In the case of the intravenous injection of tagged TMV, the auto-

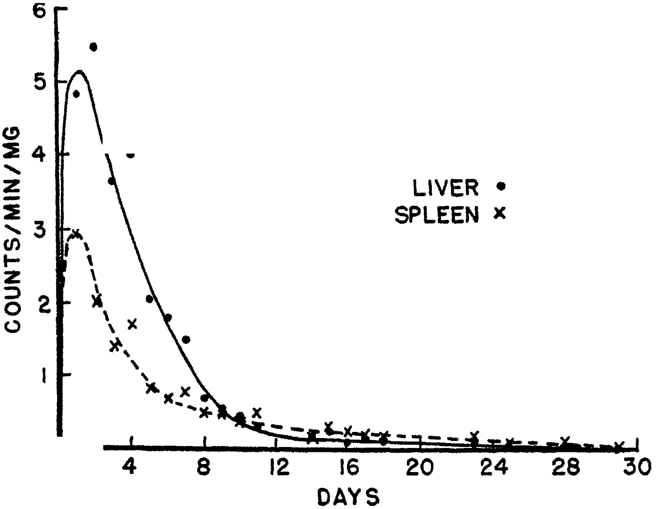

FIGURE 1. Radioactivity in the liver and spleen in counts/minute/mg. of the dry weight. (From J. Immunol. 55: 15-26. 1947.)

radiograph was, as would probably be expected, markedly different from $Na_2HP^*O_4$, since the only tissue readily observable was the liver.

FIGURE 1 shows the radioactivity in counts per minute per milligram that was found in the liver and spleen over a period of 30 days after the intravenous injection of tagged TMV. The highest counts were observed in the 24-hour samples, which, however, does not imply that this was the peak, since no determinations were made prior to this time. The radioactivity in both tissues dropped rapidly during the first 8 to 12 days, so that by the 14th day 95 per cent of the 24-hour activity was lost. During the last 2 weeks, *i.e.*, from the 14th day on, the rate at which the radioactivity disappears is much slower, so that only 56 per cent of the radioactivity observed on the 14th day is lost. The significance of the change in the rate at which the radioactivity is lost between these two periods will be discussed subsequently.

Before continuing, one interesting observation should be made. It has been generally reported that the liver is very effective in removing foreign proteins from the blood stream, but that per milligram of tissue the spleen removes greater amounts. This does not appear to be true for the mouse and intravenously injected TMV, since at 24 hours a milligram of liver contains almost twice as much radioactivity as a milligram of spleen.

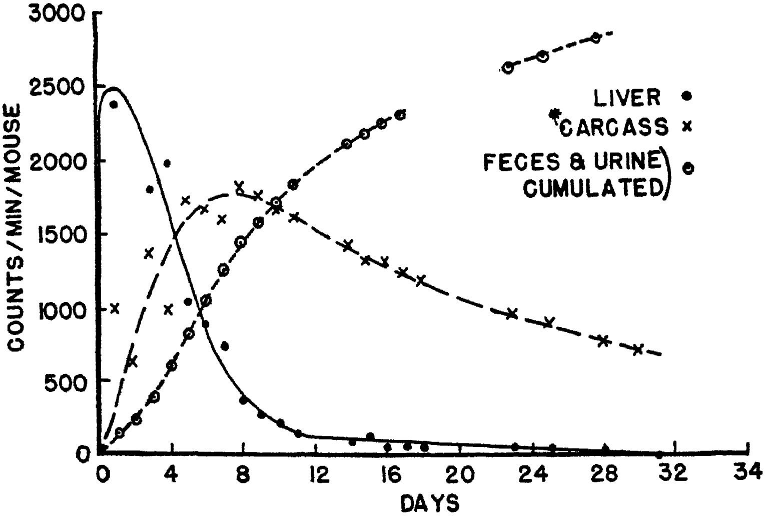

FIGURE 2. Distribution of the radioactivity per mouse in the liver and carcass (total animal minus liver and spleen), respectively, and the amount excreted in the feces and urine. (From J. Immunol. 55: 15-26. 1947.)

FIGURE 2 shows the radioactivity per mouse for the liver, for the carcass minus the liver and spleen, and the cumulated data for the radioactivity excreted in the feces and urine over a period of 30 days. Twenty-four hours after the injection of the tagged TMV, 63 per cent of the injected dose was found in the liver, 3 per cent in the spleen, 3 per cent in the feces and urine, and 30 per cent in the remainder of the carcass. As shown in FIGURES 1 and 2, the radioactivity in the liver drops off rapidly during the first 8 to 14 days, and then at a much slower rate during the remainder of the period observed. The radioactivity in the carcass (minus liver and spleen) increases up to a maximum on about the 8th day and then slowly decreases for the remainder of the period. Radioactive phosphorus is excreted at a fairly rapid rate during the first 12 days and at a slower rate for the remainder of the period.

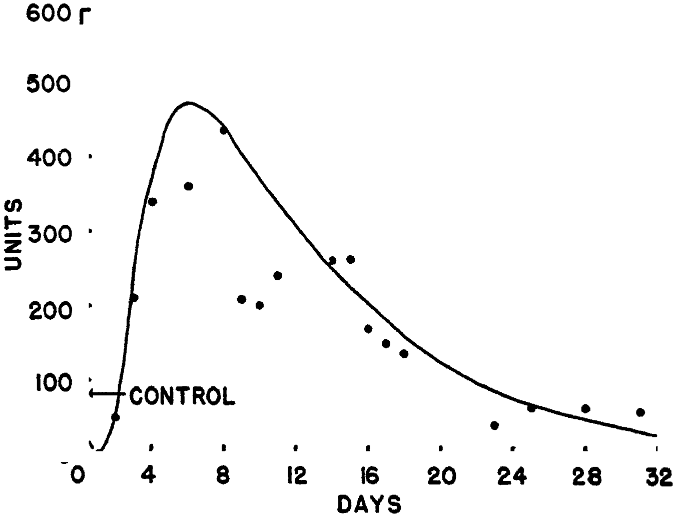

FIGURE 3. Units of TMV antibody from pooled samples of mouse serum. (From J. Immunol. 55: 15-26. 1947.)

FIGURE 3 shows the antibody produced in response to the injection of the tagged TMV. The amount of circulating antibody is expressed in terms of arbitrary units in which it is assumed that the antibody titer in the control mice equals 100 units. Twenty-four hours after the injection of the tagged TMV, no circulating antibody could be detected, while after 48 hours approximately 50 units was found. The antibody titer increased to a maximum of about 500 units between the 6th and 7th days. From this point on, the amount of circulating antibody decreased.

It is obvious that there was considerable fluctuation in the amount of circulating antibody, even though the units represent the values obtained on pooled serum samples from 5 mice. However, it is probable that the curve represents the relative day-to-day values.

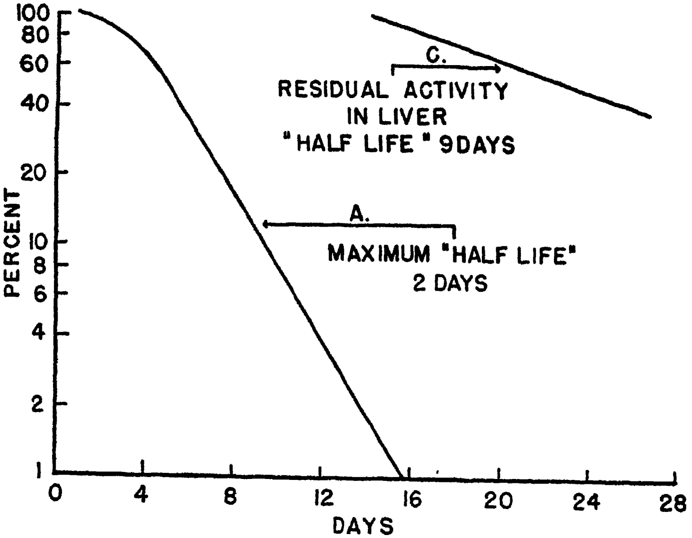

FIGURE 4. A, maximum "half-life" of the TMV calculated from a log plot of the residual radioactivity in the liver, expressed as a percentage of the 24-hour activity *versus* time. C, "half-life" of the residual activity remaining in the liver after the fourteenth day. (From J. Immunol. 55: 15-26. 1947.)

FIGURE 4 shows the calculated half-life for the tagged TMV in the mouse. Curve A is a logarithmic plot of the liver count for the first 11 days expressed as a percentage of the 24-hour count. This corresponds to the period in which there was a very rapid loss in activity in the liver and spleen. From about the 5th to the 11th day, we obtain a straight line relationship and, from this linear portion of the curve, one can calculate the maximal half-life of the TMV. What is meant by half-life in this instance is the time required for 50 per cent of the TMV to be broken down or destroyed. As indicated, the half-life is about 2 days. This calculated half-life of 2 days is a maximum value, since the radioactivity measured in the liver includes a certain amount of radiophosphorus that has been re-utilized by the mouse and incorporated into naturally occurring hepatic and splenic phosphorus compounds. That the mouse can and does re-utilize some of the phosphorus from the TMV, was demonstrated by obtaining lipid extracts of livers at various times after the 14th day. The extracted lipid fraction contained from 10 to 20 per cent of the total radioactivity found in the liver. Since the TMV does not contain lipids, it is evident that the mouse has used some of the breakdown products for the synthesis of naturally occurring phosphorus compounds.

The residual radioactivity found in the liver and spleen after the 14th day is probably entirely due to re-utilized phosphorus and, as shown in curve C, it has an entirely different half-life, *i.e.*, 9 days instead of the 2 days calculated for the TMV. Curve C is a logarithmic plot of the liver count from the 14th to the 28th day, expressed as a percentage of the count on the 14th day. As one might expect, the 9-day half-life for the residual activity approximates closely the rate of excretion of radioactivity during this period.

Because of the re-utilization of phosphorus by the mouse, it is difficult to define the exact time for the complete breakdown of the TMV. However, it is possible to postulate from the data that more than 99 per cent of the injected virus had been broken down by the 14th day.

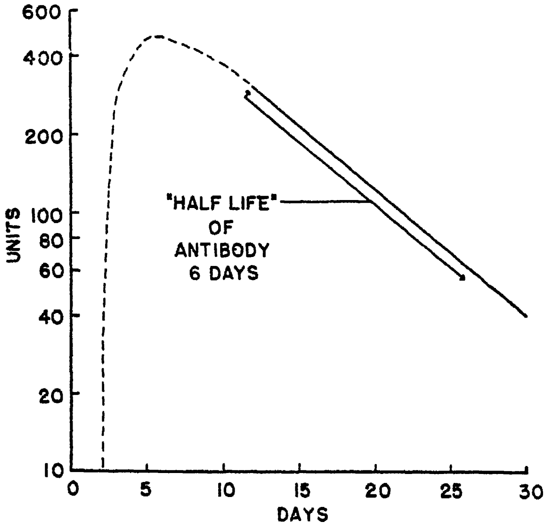

FIGURE 5. Half-life of the antibody molecule calculated from a log plot of the units against time. (From J. Immunol. 55: 15-26. 1947.)

FIGURE 5 shows the units of antibody plotted logarithmically against time. From the straight line portion of the plot, *i.e.*, from the 12th day on, one may calculate the half-life of the TMV as shown, which is about 6 days. This 6-day half-life assumes that there is a negligible amount of antibody produced after the 12th day.

It should be noted that there is an approximate coincidence between the time of the probably complete breakdown of the TMV, *i.e.*, by the 14th day, and the linear decrease in circulating antibody from the 12th day on. This coincidence could be interpreted to indicate that antigen must be present in the antibody-forming tissues in order to have continuing antibody formation.

In conclusion, there are a number of things that should be pointed out. First, only the nucleic-acid portion of the TMV molecule was tagged and, therefore, we have no measure of the fate of the protein fraction. However, it is probable that the destruction of the TMV molecule would parallel the breakdown of the nucleic-acid portion of the molecule. Second, since the liver took up a major portion of the injected TMV, it is assumed that the fate of the virus in other tissues of the reticuloendothelial system parallels its fate in this organ. Some support is provided for this assumption in that the rate of disappearance of radioactivity in the spleen approximately parallels that found for the liver.

Third, it should be noted that Heidelberger[5] has reported a 14-day half-life for the antibody molecule in the rabbit. As previously indicated, the anti-TMV appears to have a 6 to 7 day half-life in the mouse. This difference in half-lives may possibly be accounted for by the difference in the metabolic rates in the two animals.

Finally, it may be concluded that tagged antigens offer a very real and a very interesting tool for the solution of some of the unsolved problems in immunochemistry.

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SECTION OF PSYCHOLOGY

April 21, 1947

Doctor Anne Roe, New York, N. Y.: *Personality and Vocation.*

Before beginning this discussion of why people take on the jobs they do and why they go about them the way they do, I should like, first, to ask the question, "Why do people work?" The obvious answer, that people work to earn a living, is by no means the whole story. Of course, it is a fact, but I should like to call to your attention the further fact that there are cultures in which the problem of earning a living is a very different matter and occupies much less of the individual's time and energy. In our culture, just "living" is a pretty complicated affair, which makes earning it more complicated, and this makes living more complicated, and so it goes on. How we ever got that way is a question I shall avoid discussing at the moment. Instead, let us consider some aspects of the present situation which have a bearing on the problem before us, and in order not to complicate the discussion unbearably, assume that we are considering so-called normal times with a good level of employment.

Let us note, first, the amount of time we spend working, which we take for granted. I suppose that most of us work at least seven or eight hours a day, at least five and maybe six or seven days a week at some defined job; that we spend about the same amount of time sleeping; and that everything else, eating, making love, spanking the children, playing bridge, going to the movies, cheering the Dodgers, has to come out of the rest of the 24 hours a day. It is this defined job, on which we spend so much time, with which we are concerned here.

This balance of work, sleep, and play must be satisfying to most of us (even discounting the powerful effect of having been brought up in the system), or there would be a lot more who attempt to flee to the South Seas or return to the land. And although we may day-dream of the pleasures of life with a minimum of occupation, like the little boy—

> "I wisht I was a little rock a-settin' on a hill
> A-doin' nothin' all day long but just a-settin' still
> I wouldn't eat, I wouldn't sleep, I wouldn't even wash
> But just sit down beneath a tree, and rest myself, by gosh . . . "—

I strongly suspect that it is just as well we do not get beyond the dream most of the time, or we should be afflicted with galloping boredom.

Now, boredom is an emotional state which people go to great lengths to avoid, and its cause seems to be quite simply absence of occupation. Why does this seem such an intolerable state? Lewinsky[1] remarks, "There seems to be an inner force which seeks outlet in regular and regulated occupation." She discusses relationships between sexuality, aggression, and occupation and points out that civilized society tends to keep everyone occupied and that refusal to work is taken as a sign of open aggression. To be without a job, whether due to one's own inadequacies or errors or not, is a difficult pill to swallow. Most studies of adjustment in old age point out the importance of meaningful occupation for the emotional health of the individual, emphasizing that it is often the lack of this that makes for so much unhappiness in later life. Moreover, as Kuhlen[2] points out, large numbers of adults are dissatisfied with their jobs even when the financial returns are adequate. Obviously, they had expected more of their work than pay.

It is clear, then, that there is more to working than earning a living. We should not be surprised that there is more to choosing a job than just finding one. Before discussing a theory of the nature of work and of the relationship of the individual to it, let us turn to such evidence of the nature of this relationship as is available.

An enormous amount of work has been done on the testing of aptitudes and special skills, and there has also been a great deal of work on vocational interests. However, both of these problems are only peripherally related to our present one. It is obvious that, if a given job requires a certain level of intelligence or certain special skills, any person lacking these will not perform well at the job. On the other hand, there is no guarantee that any person having them will therefore perform well at it. That the programs in industrial psychology have not gone farther than they have, seems to me to be largely because job allocation has been conceived in terms of aptitudes rather than in terms of the whole person who is to do the job. Vocational counselors themselves are realizing this more and more. Orrico[3] puts it that "Personality defects far outweigh aptitudes, however outstanding, in their influence on the success one will enjoy in a given occupation or profession." The importance of extreme variations is fairly obvious. You would not, for example, suggest that a man with claustrophobia should take a job as an elevator operator or coal miner. But phobias or obsessions are not necessarily generally disqualifying. The best housecleaning I have ever had done was the work of a man who was a severe obsessional neurotic—he happened to be obsessive about dirt.

Korner[4] has written a thoughtful report on the causes of inappropriate vocational choice, which recognizes the emotional meaning of work. But there have actually been very few studies investigating any relationship between personality and occupational choice and performance.

Vernon[5] has studied occupational choice and personality, concluding that, when the occupation is felt as an essential function of the personality, its choice is determined by various "drives" of personality, such as security, pleasure, activity, self-reliance, etc. This I am sure is true, but I should like to reduce the problem to somewhat simpler terms and shall attempt to do so later.

There have been studies of a few groups by means of personality inventories. These are pencil-and-paper tests, requiring the subject to answer a series of questions. Usually he answers yes or no to such questions as "Do you have headaches more than most?", etc.

Johnson[6] has reported that seminary students were entirely heterogeneous on this test, and that life insurance salesmen were homogeneous in only one respect—dominance. McCarthy[5] reported Catholic seminarians had more neurotic tendency, more self-consciousness, more unsatisfactory total adjustment, higher religious interests and were more submissive than other students of the same age.

Hampton[8] says that "the grocer has an average personality in no way distinguished from the large majority of people."

Dodge[9] found some traits common to successful clerical workers—they are not moody or subject to worry, are even-tempered, unwilling to accept responsibility, non-social, lack self-sufficiency, and do not crave admiration.

That nothing very profound has emerged from these studies is, in part at least, a reflection of the superficiality of the instruments employed. Recently, the Group Rorschach Method has been applied to a number of different vocational groups, chiefly to validate the method by comparison with results on individual tests. Except for a very few papers, usually with very few subjects, vocational analyses have not been made. Kaback,[11] however, tested 300 men (75 each accountants, accountancy students, pharmacists, and pharmacy students) and, after an elaborate analysis, found no clear-cut personality type in either vocation.

This brings us to my own studies on artists and scientists. They have been published separately, and any who are interested in the technical details will find them in those reports.[11] Here, I shall try to pull out the highlights of each study, offer some comparison of them, and then against that background try to indicate the lines along which I think a theory of the relationship between occupation and personality can be developed.

The study of artists was undertaken originally for the Yale School of Studies on Alcohol, and was planned to investigate any relation between use of alcohol and artistic creation, but the material was so rich in other ways that it led me rather far afield from the original purpose. I have never undertaken any study which gave me such rich personal rewards. The artists cooperated so fully, were so extremely generous with their time and their interest that it was a profoundly moving experience to work with them. I came out of it with such strong personal reassurance of the essential decency of man that all the evidence to the contrary which the last few years have produced cannot shake me.

The twenty men who were the subjects of the first study are all artists of top rank. They represent most of the major trends in present-day painting. They averaged 51 years of age at the time of the study. The data consist of interviews, study of their paintings, critical opinion about them as expressed in various art journals, and of the results of two personality tests.

These two tests were the Rorschach and the Thematic Apperception Test. I will confine discussion tonight to the results on the Rorschach. This test consists of a series of 10 ink-blots, reproduced on cards for individual administration or thrown on a screen for group administration. The task of the subject is just to tell what he sees in the cards or what they look like to him. The responses are scored for such things as the proportionate use made of the whole blot or of large or small details of the white spaces, the clarity of the forms perceived, whether or not movement is seen in the cards and of what sort this is, and what use is made of color and shading.

From this, it is possible to infer a good many things about the structure of the subject's personality. If this seems a little esoteric, let me give a simple illustration, which should be qualified by the statement that, in this method, no factor is considered by itself but only against the background of the rest of the record. We have found that, in general, a subject who customarily uses the whole card in his response, either fitting details into this or disregarding them, is one who tends to more abstract thinking, whereas one who rarely uses the whole card but picks out large obvious details is one who is much more likely to concern himself with practical, concrete matters, and is not much given to abstracting and generalizing. The first one does not see the individual trees in the forest, while the other does not see the forest for the trees.

The artists showed extreme heterogeneity in the general personality structure presented, and, to my astonishment, many of them did not give any of what we have supposed to be indications of creative ability. Though some tendencies appeared in most of the group, these did not distinguish them from all other groups, with one possible exception. The artists were characterized by above-average intelligence, unusually great use of whole responses, marked prevalence of color and shading shock, and overproduction of responses of sexual and anatomical content. They gave very few Technical Responses, only half of them giving any of these. For this group, I considered as Technical Responses—a category which I use for responses referable to the specific profession of the subject—such as "It reminds me of a painting by Kandinsky." It is probable, however, that to have so classified the anatomical responses would be more nearly analogous to the bones I called Technical Responses for the scientists. It is these that distinguish this group.

The Rorschach has been considered to offer good indications of the presence or absence of "creative ability." About half of the group, however, did not give any striking indications of this, and for some of them the indications were quite to the contrary. Yet, these men are demonstrably artists, and successful ones at that. I do not think the difficulty is only a semantic one, which leaves us with two alternatives: (1) Creative ability may exist without being shown in the Rorschach, or we may recognize some indications of it and not others; or (2) One may be a successful artist in our society without having creative ability. I am not prepared to support either alternative at this point. To those who would greet the latter with loud cheers, however, I would utter the caution that, of the six men who gave none or only one human movement response, which is one of the indicators we use for this, only one is an "abstract" painter.

Analysis also showed that, as a group, they have a characteristic emotional adaptation which is non-aggressive and rather passive in nature and somewhat more feminine than masculine by our cultural stereotypes. Lest there by any misunderstanding, let me state that the problem is not one of homosexuality, which is not present in any of this group. It has been my experience that this type of adaptation is characteristic, in our society, of the sensitive, intelligent man who follows more or less intellectual pursuits.

Choice of this vocation, so far as I was able to learn, did not result solely from special abilities, nor primarily from a particular personality pattern, nor from any disparate elements in the personality so far as we are now able to isolate them. Study of the individual lives, however, makes it clear that this profession does satisfy emotional needs for most of the men which have not been satisfied in other ways, and gives us a clue not only to the meaning of creativeness, but to the meaning of all work activity. I shall defer discussion of this point for the time being.

A prolonged attempt to discover general relationships between any Rorschach determinants and the way in which the men painted was not very successful. I can, however, offer a few generalizations, though I do so with some hesitancy. The representational and naturalistic painters do seem to be better adjusted on the whole than the others. The men who are noted as colorists are more responsive to the color cards than the others. The use of color in painting does not bear any simple relationship to personality structure, but it does appear that the anxious, cautious, and compulsive person does not use color as freely or brilliantly, or for fun. There is an association between originality in painting and creative ability as spotted by the Rorschach, and there is also an association between originality in painting and the use of white space as a determinant on the Rorschach.

In individual instances, performance on the Rorschach was often extraordinarily illuminating in considering aspects of the painting. I can mention only a few of these. One painter stated that while he has no difficulty with the general plan of a painting, he always has trouble with some little detail. One of the most characteristic things about his Rorschach was that he constructed quite good wholes, but in a number of them some small detail was either unrelated or bizarre, and the nature of these gave some clue to the source of his troubles. Another painter commented that he was not constrained by the things he saw in front of him; his Rorschach amply confirmed this. The man who used the fewest whole responses often has difficulty with making his compositions a unified whole and is in general much less concerned with this than are other men. A painter whose protocol is full of references to terrifying things in empty space customarily covers his canvas with a mass of details, leaving no large undifferentiated surfaces.

The study of scientists is much less complete. Perhaps I should specify it as a study of vertebrate paleontologists, for I do not know how far vertebrate paleontologists are representative of scientists in general.

The material I have consists of Rorschach records taken, by the group method, of members of the Society of Vertebrate Paleontology. The test was first administered at the annual meeting of the society two years ago and, later, to members of the department in the American Museum of Natural History who happened not to be present at the meeting. In this report, I excluded the records of members not full-time workers in paleontology and also the records of the women, which are very unlike those of the men. This leaves eighteen in the group at the scientific level, and nine technicians, *i.e.*, field and laboratory men. The group, though small, is actually an excellent sample of the men in this field. Differences between these groups are not great and are chiefly reflections of differences in educational and intellectual level, except that the scientists give a great many more Technical Responses.

Unlike the painters, however, this group is surprisingly homogeneous in respect to certain personality characteristics. They tend strongly to abstractions and to formalized, objective thinking, and most of them show a marked inhibition of any tendencies to project themselves into a situation. They empathize little, relatively speaking, either with things or with other people. They tend to have a rather passive emotional adaptation, like that of the painters, although the evidence here is not so decisive. Very few of them manifest any creative capacity.

I have not studied the individual work of these men for comparison with personality characteristics. I do know that one of them, only five per cent of whose responses used the whole card, does beautiful detailed descriptive work, but never generalizes. I hope, some time, to check on my suspicion that the difference between lumpers and splitters in zoological classification is associated with a difference in personality in this respect! I can add, with the caution that the groups are very small, that those whose work is the most broadly theoretical and most widely significant are among the few who do not show a severe inhibition or lack of creative capacity.

This picture is in accord with other studies. Munroe[12] found this type of protocol characteristic of girls who choose scientific subjects in college, and stated that they tend to have higher non-verbal than verbal ability on intelligence tests. It is well known from college entrance examinations that arts students excel in tests using largely non-verbal abilities. It is known, too, that women more often excel in verbal and men in non-verbal abilities. This, then, is such evidence as is available. From now on, I am hypothesizing, not concluding. I have already made the point that work has emotional importance for a man. It is rather curious that, in general, therapists have neglected this, Hendricks[13] being one of the few who have paid attention to it. He has been concerned to integrate what he perceives as the emotional importance of work into general analytic theory, and has proposed what he calls the *work principle.* He says, "Work is primarily motivated by the need for efficient use of the muscular and intellectual tools, regardless of what secondary needs a work performance may also satisfy." He considers it an expression of the "instinct to master" and notes that this is similar to what Angyal[14] calls the drive for autonomy.

Angyal postulates two major drives, defined as follows: Because of the *trend toward autonomy,*

> ". . . the person has a tendency to master the environment, and by conquest and achievement to impose his intrinsic determination upon a widening realm of events . . . In the trend toward increased autonomy the biologically chaotic items of the environment are fitted into the structure of the individual's life. In the homonomous tendency the person seeks to fit himself into larger organizations. The *trend toward homonomy* expresses the tendency of human beings to share and to participate in, to fit into and to conform with, superindividual categories such as the family, the social group, a meaningful world order, etc. Characteristic examples of the trend toward homonomy are social, religious, ethical and esthetic attitudes."

Accepting this, for the moment, as essentially correct, where does work fit into the picture? There is no question but that work, any work, offers autonomous satisfactions. It may be that some jobs or types of work offer no more than this, plus, of course, economic return, except incidentally. By that I mean that the fact of being with other people and working with them is incidental to the job itself and gives homonomous satisfactions. This is well epitomized in the cartoons about "They don't speak our language."

Other jobs, however, may be a source of homonomous satisfactions in themselves; they are jobs a man can put himself into. The extent to which this can be done varies from very little all the way to the peak which is reached at the height of creative activity. In creative activity, both drives are deeply satisfied, and its only rival in this respect is mature sexual activity. Furthermore, there seem to be differences in the way in which a man puts himself into his work. These are most easily differentiated, I think, in terms of what he empathizes with most readily, —whether with people, with things, or with ideas.

The fact that, in our society, work is a major source, if not the major source, of autonomous satisfactions, and that it may also be a major source of homonomous satisfactions, explains the dominance of vocational activities in our lives. It explains many other things. Many men have taken refuge in their work in the face of the most acute problems in family or personal relations and have been sustained thereby. To be unhappy in the work and frustrated in it, or to be without it, are major traumatizing situations. I should like to remind you that Ginzberg[15] found work relief immensely superior to other forms.

It seems reasonable to assume that there are individual differences in the strength of these two drives, both absolutely and relatively. Some selection of occupation can be expected, then, in a very general way, in so far as those who have stronger homonomous needs will gravitate into occupations which will help to satisfy these.

It is necessary here, to insert a word about sex differences. I have previously maintained that there is a fairly consistent sex difference in the relative strength of the two drives. Whether this is biological or cultural, I am not prepared to argue. In any case, autonomy has a distinctly "masculine" tinge and homonomy a "feminine" one. To a considerable extent, the men who follow many of the learned professions, and the arts, have, by old stereotypes, a somewhat more feminine emotional adaptation. It is understandable, then, why they have chosen these vocations. It is just in these that they can satisfy their greater homonomous needs.

On this basis, one might ask why there are not more women than men in any of these vocations. The simple fact is that the biological role of women, and to some extent the social role, confused as I am told the latter is at the moment, offer a multiplicity of homonomous satisfactions which men do not have. Hence, it is the woman with a relatively larger "masculine" component, or, as we have been putting it, with relatively strong autonomous drives, who is the one who enters the arts and the professions.

I have no evidence for other than professional occupations, nor sufficient personal acquaintance in other circles to be able to discuss the situation in them. At the professional level, however, it seems to me that the relative strength of autonomous and homonomous drives enters into professional choice, something in this manner. Men whose autonomous drives are strong but not markedly so, whose intellectual make-up tends to the relatively greater dominance of non-verbal ability, and whose homonomous drives seem to find satisfaction in empathy with ideas, with symbols of things rather than with the things themselves, are men who are well suited to the pursuit of science and likely to be attracted to it. In another group, where homonomous drives seem to be very much stronger, but in whom the major need is for empathy with the world outside, with things rather than with people, or with people as a special sort of thing—in this group, who need to incorporate the world into themselves and then to re-express it, we find the artists. In this same group with stronger homonomous drives, we also find a more apparently feminine emotional adaptation. In still another group, homonomous needs are very dominant, and their expression is through empathy with people. These are the men who are most feminine in their emotional adaptation and who are happiest in such professions as the ministry, teaching, social work, and medicine.

I wish to emphasize the point that this is, as yet, only a hypothesis, and that much more study is needed. I offer it only as a hypothesis now, but as one which I have found meaningful and which offers leads for further research.

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SECTION OF ANTHROPOLOGY

April 28, 1947

Doctor George F. Carter, Department of Geography, Johns Hopkins University, Baltimore, Maryland: *The Distribution of Races of Maize among the Indians of the Mississippi Valley.*

The new discoveries concerning the history of corn or maize have made possible a clearer insight into the sequence and route of diffusion of maize. This is achieved by two means: by the recognition that there are races of maize, just as there are of man, and by the study of the presence of knobs on the chromosomes of maize. The method of establishing races is very comparable to that in man. Several critical, measurable values are selected, as, for example, row number, kernel shape, ear shape, stem type, and so forth. Knobs on the chromosomes of maize have proved to be of great utility. These markers are generally believed to be the result of the crossing of Andean maize with *Tripsacum*. In North America, most maize carries a considerable number of these markers, and the variation in number and their distribution on the various chromosomes have been found to have significant geographic distributions.

The material on which this study is based is the Department of Agriculture's collection of Indian maize. A chromosomal study of this material has been published by A. E. Longley.[1] In addition, I have taken 12 measurements on each of 350 ears from 27 Mississippi valley tribes. Both sets of data have been mapped. The resultant distributions are then interpreted in terms of the theory of age and area. The results indicate multiple introduction of differing races of maize into the United States. These introductions must have differed both in time and in route. The east of the United States differs in most characters from the southwest. Some southwestern tribes, however, share definite chromosomal characteristics with the east. Within the southwest, there are also recognizable differences. The Pima differ in many characters from the Pueblo. The Hopi and Zuni often differ from the other Pueblo groups. Other unusual relationships that were brought out are the resemblance of the Ute to the Northern Plains; the presence of two distinct strains of maize among the Pawnee, one characteristic of the Pueblos and the other of the Plains; the peripheral and early character of the maize of mountain Cherokee; and other characteristics.

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[1] **Longley, A. E.** Chromosomes of maize from North American Indians. J. Agric. Res. 56 (3): 177–196. 1938.

Attention is called to the utility of this means of working out relationships of peoples and of routes of diffusion. Anthropologists are urged to pay more attention to the cultivated crops that native peoples are growing.

SECTION OF PHYSICS AND CHEMISTRY

April 25, 1947

Mr. Gilbert D. Miles, Colgate-Palmolive-Peet Company, Jersey City, N. J.: *Interfacial Phenomena Associated with Mixtures of Surface Active Materials.*

The following is an outline of the various aspects of the problem which were discussed, together with the references to the literature from which the specific data were gathered for the presentation.

I. Minima in surface or interfacial tension concentration curves which are characteristic of mixtures of certain surface active materials.[1, 2]

II. The selective adsorption of certain components from solutions containing mixtures of surface active materials at air/liquid or benzene/liquid interfaces as indicated by foam or emulsion extraction.[2]

III. The influence of mixtures of surface active materials on the flow characteristics of bulk liquid through foam.[3]

IV. The effect of mixtures of surface active materials on foam stability.[4]

Conclusions

The surface or interfacial tension concentration curves for solutions of pure sodium alcohol sulfates exhibit minima when small amounts of the appropriate homologue are present.

The surface tension concentration curves for solutions of sodium alcohol sulfates exhibit minima when small amounts of certain water-insoluble surface active agents are added.

Foam or emulsion extraction experiments have demonstrated that the ratio of the concentrations of the surface active components in the surface compared to the solution, changes abruptly in the region of the minimum. This is in the direction of increasing concentration of the minor component in the interface at the minimum.

Small amounts of water-insoluble surface active materials decrease the rate of flow of liquid through foam compared to the solution without this addition, although the bulk viscosity may be only slightly different. There is a temperature for these systems above which the flow rate is not affected by the addition of the water-insoluble material.

In certain cases, solutions containing two surface active components exhibit foam stability of a higher order than is shown by solutions of either component alone.

DISCUSSION

From the concept which was advanced elsewhere,[1] the surface properties of mixed alcohol sulfates can be explained as special cases of a "salt effect." It is conceivable that at least two types of mixtures may show no minimum at all:

(A) Mixtures of alcohol sulfates when the mole ratio of the components is high.

(B) Mixtures where the surface activity is nearly the same for each component alone.

It is also probable that the lowest surface tension obtainable with mixtures of alcohol sulfates will not fall below the lowest value obtainable with either component alone.

Aside from these points, it might be well to reconsider, in closing, the techniques themselves, and what part they have played in these studies of mixed surface active materials.

(1) Surface and interfacial tension measurements can, in some instances, be used as a sensitive test for the presence of traces of a second surface active material in a solution.

(2) Foam and emulsion extraction guided by surface tension measurements can be useful in the purification of surface active materials, and in measuring the surface concentration of one of the surface active components in a mixture.

(3) Data on the flow of bulk liquid through foam can be useful in gaining some insight into the flow characteristics of surface films of mixed as well as single surface active materials.

(4) Foam stability measurements are affected by surface phenomena which include properties not measured by the other three methods and, as such, they deserve to be included as part of such studies.

The use of these methods has provided information regarding the properties of mixtures of surface active materials which has been exceedingly useful in obtaining better insight into these phenomena.

Certainly, the theoretical significance previously attached to the appearance of minima in surface tension concentration curves must be questioned in view of the fact that they may merely be a manifestation associated with systems containing more than one surface active component and, therefore, outside the scope of Gibbs' theorem.

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MR. J. J. BIKERMAN, Merck and Company, Inc., Rahway, N. J.: *Tackiness and Adhesion.** (This lecture was illustrated by lantern slides.)

The interatomic forces known to us are definite functions of the interatomic distance. This means that if two neighboring rows of atoms in, *e.g.*, a metal wire or a glass rod are separated by force—that is, if the wire or rod is broken—and then brought in contact again, the attraction between them should be just as strong as before rupture. If this conclusion were confirmed by experience, there would have been no use for adhesives.

In reality, of course, the fragments of a broken wire or a broken rod do not stick to each other when brought together. This absence of sticking has two general reasons. (a) Every surface freshly formed in air immediately becomes coated with an adsorbed film of oxygen, water, etc. When the new surfaces are made to "touch" each other, the contact is established only between oxygen and oxygen, or between water and water, and, consequently, is readily broken. (b) All solid surfaces are rough and therefore can touch each other only in few discrete points, even if there is no gas adsorption (as in an exceptionally good vacuum).

Gas adsorption is a chemical, and surface roughness a physical phenomenon. The main purpose of this paper is to estimate the relative importance of chemical (or molecular) and physical (or rheological) factors in the action of adhesives.

Corresponding to the above causes of the absence of sticking, the adhesive must (a) displace the weak adsorption layers from the surface of the solids to be joined, and (b) fill in the depressions on both solid surfaces and form a continuous phase between them. Process (a) has a chemical, and process (b) a physical nature.

When the clearance between two solids is filled with a liquid adhesive, a greater effort must be expended for separating the solids than when there was adsorbed oxygen or water between them. This effort is called *tackiness.*

Tackiness is, in all probability, a purely rheological phenomenon. Such was the conclusion reached by J. Stefan as early as 1874. He measured the tension $F(=$ force per area) required to separate two plates jointed by a liquid adhesive, within t seconds, and found the equation

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* This address was delivered before the meeting of the Section of Physics and Chemistry on March 28, 1947.

$$Ft = \frac{3}{4}\pi\eta a^2\left(\frac{1}{h_1^2} - \frac{1}{h_2^2}\right),$$

Here, η is the viscosity of the adhesive, a is the radius of the plates (or of the smaller plate if they are not equal), h_1 is the original distance of the plates and h_2 the distance reached after t seconds of application of the stress F. Usually, $h_2 \gg h_1$, and the term $\frac{1}{h_2^2}$ can be neglected in comparison with $\frac{1}{h_1^2}$.

Stefan's equation shows that there is no definite minimum force below which no separation takes place. If t is long enough, even a very small stress F will achieve separation. If molecular attraction had to be overcome before separation is possible, a minimum value of F would have been observed. Neither the properties of the solids nor the molecular forces between the solids and the adhesive affect the value of Ft. This value is determined by the geometry of the experiment (*i.e.*, the magnitudes a, h_1 and h_2) and by the viscosity of the liquid.

Stefan's equation has three interesting limitations:

(1) Its derivation assumes that the viscosity η is "normal," that is, that the adhesive is a Newtonian liquid. A corresponding equation applicable to "abnormal" liquids, and repetition of Stefan's experiments using "abnormal" liquids, would be welcome.

(2) By making t small, F can be made as large as desired. However, a large F can be greater than the tensile strength of one of the solids jointed. In this case, the solid breaks before the liquid has time to flow. This effect is easily realized by rapid peeling-off of a paper or cardboard strip recently glued to a solid.

(3) By making η very large, the product Ft can be made very large. A simple calculation shows that, in an apparatus of ordinary laboratory dimensions, many bitumen or rosin adhesives would give rise to products Ft so large that the joint would be stronger than copper or even steel for as much as several minutes. Here, again, the experience is quite different. Bitumen or rosin joints break long before the tensile strength of copper or steel is reached. The adhesive breaks as a brittle solid although, as experiments using small stresses prove, it is a Newtonian liquid.

A simple apparatus was constructed* to study the conditions which determine whether a uniform phase behaves as a solid or as a liquid. It is called *penetroviscometer*, and consists of a glass tube partly filled with adhesive and a cylindrical brass rod which can move within the tube along its axis. When the brass rod descends in the adhesive (because of gravitation), the rate of its movement is determined by the viscosity of the latter. This viscosity can be calculated from the rate of descent and proves to be independent of stress. When, subsequently, the tube is inverted so that the brass rod gradually slides out of the tube, the tackiness is measured (see the definition given above).

On the theory that tackiness is determined by viscosity, it is possible to compute the rate of sliding out. It proves to be smaller than the experimental rate. The cause of discrepancy can be seen. The rate of flow of the adhesive (upward, *i.e.*, toward the immersed end of the rod) is proportional to the ratio $(p_0 - p)/l$, p_0 being the atmospheric pressure, p the smaller pressure above the cylinder, and l the height of the column of the adhesive. Because of the irregularity of the adhesive-air interface, the height l is not constant along the circumference of the rod. The regions of a smaller l, that is, those above an indentation, show a greater rate of movement than other regions. This means that an indentation gets deeper, and its depth increases at an increasing rate. Finally, air reaches the upper end of the rod, and the rod falls out, when a large part of the adhesive is still included in the annular space between the tube and the rod. Apparently, a liquid behaves as a solid when disruptive forces applied to it are much greater than the forces of surface tension, gravitation, and similar factors which tend to preserve the uniformity of the phase.

The viscosity of the usual adhesives gradually increases after application, because of cooling, evaporation, imbibition or chemical reaction. When the viscosity reaches a nearly constant value, we say that the adhesive has set. The time interval between the application of an adhesive and the stabilization of its viscosity is termed *setting time*. During the setting time, many misfortunes can befall the adhesive film. If, however, no misfortune occurs, at the end of setting the adhesive forms a continuous solid bond between the members jointed.

When the effort required for rupturing the set joint, *i.e.*, the *final strength* of the joint, is measured, the result is different according to where the rupture takes place. One of the solids jointed can break: in this case, its tensile strength is the magnitude determined in the experiment. Or, the rupture can pass through one of the surface layers between the solids and the adhesive film: in this case, the experiments usually are poorly reproducible. Finally, the adhesive itself can give way. When this happens, the tensile strength of the adhesive film is the relevant magnitude. As a rule, the attractive forces between the molecules of the adhesive and those of the solids are not determined when the final strength is measured.

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* In the laboratories of The Printing and Allied Trades Research Association, London, England.

It was believed that, even when the rupture occurs wholly within the adhesive film, the final strength is different from the tensile strength of the adhesive, because experiments show this final strength usually to be greater than the tensile strength of a free specimen of an identical adhesive, and also to depend on the thickness of the film. However, the external differences between final strength and tensile strength are no sign of their different nature. These differences are due to the different dimensions of the test pieces. The rule, "the thinner the joint, the higher its strength," is a consequence of the other well-known rule stating that "the shorter the specimen, the greater its breaking load."

The higher breaking load of a shorter specimen is accounted for by the smaller probability of a very weak spot being present in a smaller volume. This probability theory of tensile strength can be proved in a simple way. Consider two identical specimens, 10 cm. long. One of these is tested directly, and the breaking load corresponding to 10 cm. length is determined. The other is cut into 10 segments, each 1 cm. long. Their testing yields 10 values of breaking load, of which 9 are greater than, and one is equal to, the breaking load of the long specimen, because one of the segments contained the weakest spot of the long test piece. This means that, on the average, the lowest breaking load observed in a batch of 10 segments of 1 cm. should be equal to the breaking load of 10 cm. long specimens. Unpublished measurements on glass filaments prove this theory in a quantitative way.

The above method was applied to paraffin wax joints between a steel rod and a brass plate. The joints were either 0.5 or 0.05 mm. thick (approximately). The lowest strengths in batches of 10 thin joints were somewhat greater than the strengths of 0.5 mm. joints, so that there was no quantitative agreement between theory and experiment. The small disagreement observed is attributed to the different conditions of setting (cooling) of the wax films of different thicknesses. The thicker the film, the longer its cooling time and, presumably, the coarser the crystal grains in it. The effect of the setting conditions on the crystal size of the solidified film presumably accounts for the dependence of the final strength of a joint on the nature of the solids jointed. For instance, the rate of cooling would depend on the heat conductivity of these solids.*

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* For a more detailed discussion of tackiness and adhesion, see J. Colloid Sci. 2: 163. 1947.

SECTION OF BIOLOGY

April 25 and 26, 1947

Conference on *"Allergy."*

The Section of Biology held a Conference on "Allergy." Doctor Arthur F. Coca, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, April 25

Morning Session. "The Pharmacodynamics of Histamine," by Bradford N. Craver, Ciba Pharmaceutical Products, Inc., Summit, N. J.

"Experimental Anaphylaxis in Lower Animals: Fundamental Concepts of Specific Sensivity," by Beatrice Seegal, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Allergic Disease in Lower Animals: Classification of Allergic Disease," by Lester Reddin, Jr., Merck and Company, Rahway, N. J.

Afternoon Session. "Serum Sickness," by Samuel Karelitz, Mount Sinai Hospital, New York, N. Y.

"Contact Allergy (Skin)," by Max Grolnick, Jewish Hospital, Brooklyn, N. Y.

Saturday, April 26

Morning Session. "Atopic Allergy: Reaginic Sensitivity," by Matthew Walzer, Jewish Hospital, Brooklyn, N. Y.

"Allergy of Infection: Relation to Immunity," by Beatrice Seegal, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Allergy of Infection: Fungi and Fungous Products," by Marion B. Sulzberger, Skin and Cancer Unit, New York Post-Graduate Hospital, New York, N. Y.

Afternoon Session. "Familial Nonreaginic Allergy," by Milo G. Meyer, Michigan City, Indiana.

"Familial Nonreaginic Allergy as a Predisposing Cause of Common Cold," by Arthur P. Locke, Zonite Products, Inc., New Brunswick, N. J.

"The Antiallergic Action of Sympathectomy," by Arthur F. Coca, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

NEW MEMBERS

ELECTED APRIL 24, 1947

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Angel, J. Lawrence, Ph.D., Anthropology. Associate in Physical Anthropology and Anatomy, Jefferson Medical College, Philadelphia, Pa.

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Axelrod, A. E., Ph.D., Biochemistry. Research Biochemist, Western Pennsylvania Hospital; Assistant Professor of Chemistry, University of Pittsburgh, Pittsburgh, Pa.

Baiardi, John C., M.A., Physiology. Professor and Chairman, Department of Biology, Saint Francis College, Brooklyn, N. Y.

Bassen, Frank A., M.D. Hematologist, Beth Israel Hospital; Adjunct in Medicine for Hematology, Mount Sinai Hospital, New York, N. Y.

Bassett, David Lee, M D., Anatomy of Vascular System, Histology of Ovary. Assistant Professor of Anatomy, Stanford University, Palo Alto, Calif.

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Blair, Francis Wesley, M.S., Chemistry of Fats, Oils, Glycerine and Detergents. Director, Chemical Division, The Procter and Gamble Company, Ivorydale, Ohio.

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

Ser. II, Vol. 10 November, 1947 No. 1

SECTION OF BIOLOGY*

NEW METHODS AS TEACHING AIDS IN SCIENCE

By AUSTIN JOYNER†

A method of using visual aid in the teaching of elementary scientific subjects has elicited interest. No claim is made that there is anything actually new in the method demonstrated. However, it is felt that this method and the manner in which it is used, represent a distinct advance in the difficult task of visualization of scientific subjects.

The materials consist of a board approximately the size of an average blackboard which is constructed in sections so that they can be disconnected and made portable. The face of the board is covered with a soft, black, woollen cloth which has been tightly stretched and is attached at the edges of the board. The symbols or pictures which illustrate the subject being discussed are drawn or painted on the surface of appropriately shaped pieces of cardboard. The back face of these cardboard pictures has zig-zagged strips of sandpaper pasted on its surface.

The board is used very much in the same manner that an ordinary blackboard would be used in a class demonstration or lecture. Symbols or pictures illustrating each point in the talk are placed on the board by pressing the sandpaper surface onto the cloth surface of the board with a slight downward motion. This allows the picture to remain in position as the sandpaper catches onto the cloth surface.

The use of the board can be illustrated by using symbols and pictures which are designed to present such subjects as Immunology, Nitrogen Balance, Nutritional Aspects of Folic Acid, and Atopic Allergy. Naturally, this kind of board can be used in a fully lighted room, so that the full personality of the speaker still remains in play. Also, simplicity of design, the advantages of color, and the shape of the symbols add to the possibility of retention by the audience. The board was originated by Mr. J. Hile and Mr. William Damroth, who have also interested themselves in the commercial production of numerous lecture illustrations. This method has been extremely successful in presenting complicated scientific subjects in a simplified form to large numbers of adults who have had very little scientific training.

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*The Section of Geology held a meeting on October 6, 1947, at which Doctor L. H. Adams, Geophysical Laboratory, Carnegie Institution of Washington, Washington, D. C., presented a paper entitled, *Unsolved Problems in Geophysics*, illustrated by lantern slides. No abstract of this has been received.

†Lederle Laboratories Division, American Cyanamid Co., Pearl River, N. Y. This paper, illustrated by demonstrations of material, was presented at the meeting of the Section on October 13, 1947.

Note. The Section of Psychology held a meeting on November 20, 1947, at which Doctor Clemens E. Benda, Walter E. Fernald State School, Waverley, Massachusetts, presented a paper entitled, *The Hereditary and Non-Hereditary Mental Disorders of Infancy and Childhood*, illustrated by lantern slides. This will be published in a later number of TRANSACTIONS.

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SECTION OF ANTHROPOLOGY

ANTHROPOLOGY AND EARLY INTER-AMERICAN RELATIONS

By HARRY BERNSTEIN*

The search for common origins of the Americas, combined with the rise of ethnological interest in the native peoples, has had the effect of creating and tightening cultural ties in the western hemisphere. Study of New World anthropology and archaeology has quickly become inter-American by bringing North American scholarship closer to Latin American. It would be hard to say whether historical studies about Indian antiquities provided greater incentive for cooperation than did ethnology at the time, but certainly both together guided and influenced prevailing theories and researches. During the nineteenth century, Latin American history was never far away from Latin American ethnology. The result was that Amerindian investigation brought together the workers and societies of both continents in harmonious and constructive contact.

In spite of the difficulties that blocked direct relations during the late colonial period, North American interest in Latin American aboriginal history and society has always attracted the foremost minds in American history. From the eighteenth century to the present, the work of individuals supported by the learned societies of this country opened up to view and discussion many aspects of American Indian culture. Set off by such early American figures as Benjamin Smith Barton, Ebenezer Hazard, Jeremy Belknap, the ties of science were pushed towards Latin America by the American Philosophical Society, and by Dr. Samuel Latham Mitchill of the Lyceum of Natural History in New York (now The New York Academy of Sciences). For many years, the Latin American Indian was as well known and as much at home in Boston, Philadelphia, and New York, as he was in Central Mexico, Yucatan, and Peru.

But during these years—before 1820—Indian civilization could be known only through reading. Books and chronicles, made available by libraries, publishers, and serial reprints in newspapers, were the chief means of building up knowledge. The reasons for this are well known: travel was difficult, Latin America was politically in another universe, and the fashion of culture was to read and discuss what others had seen and observed. Not until Latin America was free from Spain, after the wars for independence, were peoples and monuments of the continent opened up to direct inspection. Field travel and experience entered into judgments of archaeology and ethnology alongside the older descriptions and assertions of written literature.

It was then that North Americans began to make common cause with Latin Americans in joint rediscovery of the antiquities of their continent. It was not easy to achieve this. Ethnological study was for a long time to be dominated by armchair authority, literary and bookish in method. Book knowledge was a sort of carry-over of humanist influences, and the historian, chronicler, and observer of Latin America held highest honors. However, between the early and present stages of Amerindian study lay the record of hard field work, checked against earlier sources and writers. The study of man in his social and geographical habitat began to grow up in an inter-American world of its own, free from the dictatorship of the literary source, yet familiar with its opinion.

To be fair about this, we must admit that a great many Americans were rather well read on the subject of Indian antiquities, even though their interest in contemporary Indian society lagged far behind. Literary materials were the last word, and individual volumes and well known collections were plentiful in the United States. In fact, many of the more celebrated books by Spaniards and Latin American colonials were available to North Americans. But other handicaps resulted from what would otherwise have been praiseworthy reading knowledge. In addition to excessive stress placed upon written statements, there was another tendency to unbalance, namely, because most Spanish and English books dealt with the Indians of Mexico and Peru, both the scholar and intellectual reader echoed the subject matter of books attracted by the ruins of highland Indian empire, which offered a more striking panorama for historians, travelers, ethnologists, and archaeologists.

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Nevertheless, it became more and more usual for those societies interested in Latin America to turn to the South American continent for men and regions which might broaden the basis of ethnology and archaeology. One of the most effective ways of promoting such ties was to give new life to an old inter-American tradition and to advance cooperation by exchange relations with Latin American societies, as well as the nomination of leading Latin American ethnologists to membership in North American societies. In Philadephia, New York, and Boston, men of scientific inclination realized again that to know Latin America it was necessary to enter into direct relations of a cultural sort. The first North American society to launch and stabilize these contacts was the American Philosophical Society in Philadelphia.

The long service to Latin American studies and inter-American cooperation rendered by this Society is but a part of Philadelphia's role in the total picture of such cooperation. But the Philosophical Society took the lead, from an early date, in furthering investigations and gathering materials upon Indian linguistics as a large part of its interest in New World origins. A more important result of its work led to the enlistment of Latin Americans as correspondents who would (and did) contribute to the Society's publications and aid its researches. Thus, the first Latin American ever to be named to membership by a scientific society in the United States was Alejandro Ramírez, a scientist and official of colonial Guatemala, elected in 1801.

Thirty-five years later, in 1836, the American Philosophical Society, in its search for materials dealing with Indian antiquities in Central America, turned to Guatemala again. Five years before John Stephens had run across the Maya vein and an archaeological rush pushed Yucatan before the world, the Philosophical Society, in its eagerness for intellectual cooperation, had already begun to reach into the enormous bowl of Guatemalan ethnological riches. It is interesting to reread the letter sent to the Philosophical Society by Manuel Gálvez, President of Guatemala, a student of Indian civilization, corresponding member of the Society, and an associate of that Juan Galindo who was to contribute articles on Guatemalan ethnology in North American scientific journals.

"Guatemala 31st May 1836.

"Sir:

"I have had the honor of receiving your esteemed letter of the 21st of July of last year. I regret very much the delay that it has experienced on its way hither, as well because I would not wish that my Silence should be considered as a want of attention on my part, as on account of the interesting nature of the Subjects to which your letter refers— It is advantageous to a Country like ours, that wants only to be better known abroad, to hold a correspondence with an Officer of learned Societies, and we consider it as an object of importance, while at the same time, it will contribute to enrich your fund of knowledge, by the information which your members will receive of our antiquities, statistics, natural history &c.

"I am preparing a Collection of printed Documents, which I shall transmit to you by the first opportunity. I have already written to the Acad-

> emy of Sciences, established here in 1832, in consequence of a decree issued by me as Governor of this State, not only that they should transmit to you the MS Grammars of Indian languages which are, no doubt, in their public library, but also that conferring upon you the title of their Corresponding Member, agreeably to their Statutes, they may carry on with you a Correspondence which, no doubt, will prove advantageous to this Country."

Grammars, dictionaries, and vocabularies went up to Philadelphia, ties of inter-American correspondence were established without government assistance, and resources in the United States on Indian linguistics were enriched. All throughout the American continent, the work of the American Philosophical Society won a regard and esteem that eased the way for researches and inter-American contacts by other societies.

Latin American ethnologists and Indianists were attracted to membership in North American groups, and almost every worthwhile scientific and learned body in the United States moved away from a supposed and mythical provincialism, towards an inter-Americanism based upon exchange of publications and corresponding memberships. It is, therefore, not surprising that the American Ethnological Society, soon after it was founded, should follow the pattern of the Lyceum of Natural History, the New York Historical Society, and the American Philosophical Society—to name only a few.

The Ethnological Society held its first meeting in 1842, and within twenty years (by 1863) there were at least seventeen known Latin Americans who had been invited to membership. In this brief, first epoch of its existence, the Ethnological Society also realized the importance of reaching into Latin American countries for supporters and correspondents. Moreover, a cross-section of its membership shows that Latin American ethnology was fairly well represented by men coming from outside of Central Mexico and Peru. The names of some leading contemporary ethnological workers in Cuba, Guatemala, Colombia, and Ecuador are revealed in addition to members from Mexico and Peru. Cuban and Colombian antiquities and ethnology were added to those of Yucatan. These steps were among the first to be taken in extending anthropology and archaeology to the whole continent, but some time had yet to pass before Brazilian aboriginal history was included. In general, Peruvian, Yucatecan, and New Granadan tribes held out the most attraction—always excepting, of course, the interest in Mexico.

The index of new directions in inter-American ethnological relations may be found in the list of Latin American members of the American Ethnological Society, as its share of cultural cooperation:

José de la Luz, Cuba, 1861
Andrés Poey, Cuba, 1861
José Barrundía, Central America, 1852
Modesto Flores, Central America, 1861
Colonel Codazzi, New Granada, 185?
A. P. Herrán, New Granada, 1845
José Obaldía, New Granada, 1861
Tomás C. Mosquera, New Granada, 1861
Ezequiel Uricoechea, New Granada, 1861
Vicente Rocafuerte, Ecuador, 1845
F. Villavicencio, Ecuador, 1861
Tomás E. Carrillo, Yucatan, Mexico, 1845
Juan Pío Perez, Yucatan, Mexico, 1845
José Pacheco, Mexico, 1863
José F. Ramírez, Mexico, 1845
Manuel Gomez Pedraza, Mexico, 1845
Mariano Rivero, Peru, 1845
Pascual de Gayangos, Spain, 1845

It should be remembered that, in the nineteenth century, there was a close affinity between history and ethnology, especially in connection with the origins and antiquities of the New World. Very often, it happened that Latin American ethnologists and Indianists, who might not be members of the American Ethnological Society, joined the work of other societies as correspondents and contributors of articles. Such was the case (to take but one example) with Juan Galindo, a member of New York and Massachusetts Historical Societies and a contributor to the American Philosophical

Society, but not a member of the Ethnological Society.

It would also be hard to say whether knowledge about Latin American Indian civilization was pushed more strenuously by historical groups than by ethnological. In New York as well as in other cultural and scientific centers of the United States, history, antiquities, and ethnology were fairly well joined. At least, most educated persons interested in one subject followed progress and discovery in the others. Few people, for example, were more persistent supporters of Dr. Carl Behrendt's work in Maya language and Yucatecan ethnology than was John Russell Bartlett, secretary of the New York Historical Society, the American Ethnological Society and, later, librarian of the John Carter Brown library with its rich possessions dealing with Latin America.

Latin American archaeologists, ethnologists, and historians who were corresponding members of the New York Historical Society have been listed elsewhere. Bartlett was among the first to extend its chain of membership down into Brazil, as will be seen. He was not much different in his acceptance of the mutual assistance pact between Latin American ethnology and history from George Gibbs, of the same two societies and later a stalwart supporter of the Smithsonian's work. The discovery of the Maya had widened the so-called provincial limits of the New York Historical Society. The nomination of Manuel Alonso Peón of Mérida, Yucatan, alongside the names of many other Yucatecan and Latin American ethnologists and archaeologists is one illustration of how that historical society followed up a fine chance to participate in Central American researches. Gibbs soon acknowledged from Peón a shipment of "curiosities" from Yucatan, but another letter from Gibbs to Bartlett shows how purposeful was the large aim of the New York Historical Society in contributing to the history of Maya civilization.

> [prob. 1848.]
>
> "Dear Bartlett:
>
> "Enclose these resolutions in a letter to Don Manuel Alonso Peon of Merida Yucatan stating that you have been informed by Mr. Jas. R. Hitchcock that he is the proper person to whom to convey the respects of the Society. Also have a diploma for Peon ready as corresponding member, and mention that in your letter that we shall be glad to receive from him any documents or antiquities, and any information as to the country, particularly works on the *Maya*. . . . Tell Peon that at a suitable opportunity when peace comes you will send him our collections.
>
> Geo. Gibbs."

The inter-American work of the New York Historical Society, it can be shown, was matched by the American Antiquarian Society, whose ethnological members from Latin American began with Juan Galindo, nominated in 1836, continued with Yucatecan members during the rest of the century, and reached a peak with the selection of Juan Ambrosetti and Samuel A. Lafone Quevedo of Argentina, in 1910. These direct connections, supplemented by exchange of documents, "curiosities," and valuable museum materials, helped to reduce the influence of literature and book descriptions upon ethnologists. But the wealth of printed accounts by travelers, missionaries, and chroniclers was not to be wasted. Indeed, the growing number of books gave rise to some useful summaries of existing knowledge. One of these résumés is Albert Gallatin's *"Notes on the Semi-Civilized Nations of Mexico, Yucatan, and Central America,"* first published in the *Transactions* of the American Ethnological Society, but also separately printed. Gallatin, president and chief benefactor of the Ethnological Society, brought together in one place the judgments of Spanish historians, some of Prescott's views, and the more recent revelations of John Stephens.

Literature and language always go together, and the literary approach of Latin American ethnology in the United States is clearly evident in the study of Gallatin. Confining his attention to the same tribes of the central Mexican plateau which had always been a focus of study, he reviewed the Mexican number system, calendar, and other inventions, but gave special emphasis and attention to the philology of the Aztec language. He had little interest in, or knowledge of, the Indian of northern and southern Mexico. The study of vocabulary and Indian grammar, already undertaken by Duponceau for the American Philosophical Society, was a subject of wide interest, and Gallatin's monograph aided in tying ethnology to languages. The stress upon folklore, legend and myth was to come later, but ethnology, like history, reflected the humanist traditions of Western Europe and, to a large extent, became a branch of literature. Letters and history, and letters and ethnology, were paired in that way.

It was hoped that the structure of language might lead to the essential unity of language, and because of this, missionary interest in grammar added greatly to the search for grammars and a linguistic key which would make it easier to reach the native. The missionary, however, was usually a field worker and his use for language had to be practical. The search for the one world of American man through a common stock of speech left a mark upon ethnological studies which had the humanist, bookish interest of the Renaissance heritage on one hand, and, on the other, the related admiration for the laboriously compiled grammars and dictionaries of the Spanish missionaries. The Trustees of the Astor Library made their collection of Indian grammars an emphatic point in their report to the New York State Assembly (1853), stating that their collection, which was later to become the New York Public Library,

> "has also the best of the vocabularies of the different dialects of the Mexican and South American Indians, which were collected and published by the early Spanish missionary priests. Books of this last class have become excessively rare and consequently dear. A perfect copy of Molina's *Arte de la Lengua Mexicana* cannot be had for less than fifty dollars; and Rincon's *Grammar* of the same language, a mean little duodecimo, bound, or rather, done up, in a limp vellum, which few would accept as a gift, costs much more than its weight in gold."

Not all American workers in Indianist ethnology, however, were in agreement as to the importance of language and philology. Some felt that language was an instrument, rather than an end; others found that architecture was a better index; a few agreed that the physical appearance of man threw much more light upon the Indian past. On the subject of classification of Indians by physical measurement, no one was more certain of the defects of linguistics as compared to cranial structure, than was Dr. Samuel G. Morton, of the Academy of Natural Sciences at Philadelphia. Morton, a medical man, approached the problem of the unity of the American Indian from a physical-anatomical viewpoint. His search for crania and his researches into measurement brought him fame, specimens, and correspondence with Latin America. His conclusions led him to disagree sharply with the philologists, and, in a letter of 1847 to John Bartlett of the American Ethnological Society, Morton declared flatly,

> "You may rely upon it, philology, however important in Ethnology, is not infrequently a broken reed."

The blow at the linguists was coincident with the strengthening of Latin American prehistory through the rise of Latin American archaeology as an explanatory study of the Indian past.

At the same time that Gallatin was summing up the known linguistic ethnology of the Nahuatl stock in Anahuac, John Stephens lived with the Indians of Yucatan in the shadow of the old ruins, and Brantz Mayer went out many times to see the remains of the ancient architecture of the Valley of Mexico. Archaeology was to have its say about the nature of Indian antiquity and the possible unity or manifold variations of man in America. American prehistory—without books or written accounts—supplied new sources of knowledge, inter-American interest, scientific debate, and cultural cooperation. The apparently sudden introduction of the scholarly world to the ruins of the Maya was combined with the renewal of interest in Aztec and Peruvian architecture alongside of the study of Indian languages.

Language, nevertheless, had great importance in bringing about cooperative study of common origins. White civilization in North and South America, which varied so much in speech, government, and culture, had to turn to the Indian for evidences of unity in the New World. Linguistics and architecture were parallel pillars which supported knowledge. Both archaeology and ethnology could have made use of language studies, although not perhaps to the exclusive extent then in vogue. All the same, there is something to be said for direct experience as required by archaeology; field observation seems to have prevented Latin American ethnology from becoming an adjunct of library research.

When travel opportunity permitted Americans to become familiar with Mexico through first-hand experience, long residence, and immediate research, there was no doubt that field ethnology, archaeology, and cultural relations gained considerably. But the hold of literary ethnology was strongly maintained for a long time. There was a general feeling that the Indian in his nineteenth century circumstances was only a poor (and uninteresting) copy of the original who had been so impressively described by historians and eye-witness observers two and more centuries before. If we take as an example of this state of mind the notices of Joel Poinsett in his *Notes of Mexico*, or those of Dr. William Keating (a resident of Mexico), we find that both of these men took a greater interest in the visible remains of Aztec monuments than in the living groups of Indians around them. The same can be said of Brantz Mayer, who set off the value of architectural archaeology against linguistic ethnology, but only insofar as it threw light upon the past, instead of the present.

Strangely enough, history and sources, whether literary or monumental, were intended to describe origins and the past, rather than to explain the present by means of the past. This reverse directional trend, which turned away from those influences exerting change upon Indian life, made it possible to link Latin American history with ethnology where primary written sources were involved, but had the inevitable effect of retarding the growth of field observation. Progress was very slow from the time of Benjamin Smith Barton to that of Daniel G. Brinton, a century later, in the process of study of the present-day Indians.

However, one of the effective ways of overcoming excessive dependence upon books was to tap the scientific personnel of Latin American ethnology and bring leading authorities into membership in North American societies. The archaeological discoveries of the Maya in Yucatan not only stirred public taste and interest, but opened the way for the American Ethnological Society to choose as its earliest members the Maya specialists, Juan Pío Perez and Tomás Carrillo. It is thus true that Latin American archaeological interest gave new life to those societies interested in the Indian, but the re-discovery also renewed an older difficulty by concentrating upon dramatic and colorful monuments in Mexico, Yucatan, and Peru, which could prove the advancement of the American Indian civilization to a plane somewhat comparable to those recently found in Egypt and elsewhere in the Middle East. While this concentration of effort brought about an increase of knowledge concerning the major cultures, its long-range effect was to hold back from continental horizons and to make investigation excessively regional.

In the general awakening of interest in Yucatan, no one can question the service of Stephens, but no one ought to deny a rightful share of credit to a little-known contemporary, Benjamin Moore Norman, of New Orleans. While Norman was "rambling," as he calls it, through Yucatan and the Maya country, he ran across the ruins and set down on paper some useful field descriptions of Chichen-Itzá and Uxmal. His book, *"Rambles in Yucatan; or Notes of Travel through the Peninsula, including a Visit to the Remarkable Ruins of Chi-chen, Kabah, Zayi, and Uxmal,"* first appeared in 1842, the same year in which the American Ethnological Society held its first meeting. Norman, a most alert and intellectually astute "tourist," became as enthusiastic about his findings as did John Stephens, and brought his discovery to the attention of Dr. Francis Hawks in New York City.

Dr. Hawks was many things to many men: a theologian, student of Spanish literature, translator of the Rivero-Tschudi account of the Indians of Peru, historian, geographer, and ethnologist. He was an influential member of the American Ethnological Society, the New York Historical Society, and the American Geographical Society, all of which had an interest in aboriginal antiquities in Latin America. The "idols" and statuary which Norman shipped to Hawks in New York were as important a collection of objects as the Mexican material which Joel R. Poinsett had earlier given to the American Philosophical Society. Far from the world of scholarship, societies, and institutions, Norman, in his own way, had given that world a first-hand sketch, plan, and survey of the Chichen-Itzá ruins. Though he still sought the support of learned societies, he contributed as an intelligent traveler to a field in which the names of Stephens and Squier rank much higher. This is probably the reason why he wanted the backing of Dr. Hawks, to whom he wrote in a letter dated New Orleans, April 25, 1850:

> "I have shipped my collection of Idols etc to New York, but I have not yet fully decided what to do with them; this will be a matter requiring your action which I shall avail myself of when I have the pleasure of meeting you again. The arrival of the idols in New York will afford a good opportunity to revive the subject of American antiquities."

Where these objects were ultimately deposited is hard to say now. Like the specimens sent to the New York Historical Society by Manuel Alonso Peón, they probably exist in one of the City's museums or have been destroyed. Norman's efforts remain obscure, and the contacts he initiated were taken up by the historical and ethnological societies.

Norman had believed that Hawks, knowing Spanish and interested in aboriginal America, would write or promote a study upon "American Antiquities." While Hawks knew as much as anyone about Latin American ethnology, the work never appeared, and the analysis of the American past was separately initiated by Brantz Mayer, historian, diplomat, and archaeologist. Mayer worked on Mexican (that is, Central Mexican) architectural remains. Of Baltimore background, he had been assigned to the Mexican Legation some years after Joel Poinsett had left and some years before Buckingham Smith, another student of Mexican history and ethnology, was assigned to that Legation. Mayer's interest in Mexican antiquities dates from his residence there, and he took up very seriously the task of learning about the country to which he had been assigned. He was one of the first Americans to sense intelligently the Indian element as part of national Mexico, yet his views differed from those of the ethnologists and historians.

To Mayer, both written record and living descendant meant much less than art and architecture as broad chronological and cultural indices of pre-Cortesian Mexico. His judgments were far more confident than the hesitant conjectures of Norman, with an emphasis on archaeology just as strong as the linguistic stand taken by Gallatin in his "*Notes*." As far as Mayer was concerned, the bookish source and the linguistic ethnologist lacked a great deal compared to the vivid record of architecture. Unlike Prescott, Brantz Mayer lived and traveled in Mexico, and was one of the earliest North American correspondents of the respectable *Sociedad de Geografia y Estadistica*. A historian of Mexico in his own right, Mayer's view, which fell upon deaf ears, was that the lamented loss of Mexican and Maya books, that is, the codices, did not in any way undermine knowledge of Indian civilization. He was convinced that the so-called codices and the "hieroglyph" documents were in no way historical, but rather *aide-mémoires* useful only in so far as they once suggested to native experts the dates, monarchs, and place-names of their past.

Boldly departing from the strong interest of his day in philology and linguistics, Mayer hinted that the latter trend was but an echo of 19th century romantic interest in ballads, grammar, folk-lore, and supernatural tales. This could not supply a stable and reliable source, and while it might be of interest to an ethnologist concerned with myth, he held that archaeology alone, recording the permanent contribution of aboriginal architecture, gave a unique key to the Indian past.

For some time to come, the work of Mayer, Norman, Stephens, and Squier continued to stir scholarly and public interest, but at the same time ethnology held its own in the study of Indian culture. The undiminished concern of Latin American ethnology for New World man owes much to the work of the American Ethnological Society. Ethnology faced a hard task if it wanted to hold its own against research into the more impressive monuments of Mexico and Peru, and include the antiquities of an entire continent. While its success was slow in this displacement process, success nevertheless did come. The Society began to take notice of ethnological and archaeological regions outside of Mexico and Peru, and to survey the civilization of all Latin America.

The secretary of the Society, John Russell Bartlett, reviewing the progress of ethnology in 1843, brought to view a good deal about investigations in all of South America. Although Bartlett kept up the good tradition by never seeing Latin America himself, he did much to distribute interest by directing it to Indianist work in Chile, Argentina, Peru, Venezuela, and Brazil. Not only did he bring the American Ethnological Society into official, institutional relations with Brazilian ethnology, but he also brought the attention of Americans to the Castelnau scientific expedition, then crossing the Matto Grosso from Sao Paulo, en route to Bolivia.

Combining archaeology with ethnology, the early *Transactions* of the Ethnological Society opened the door to the rest of Latin America with such articles as:

(1) An account of certain antiquities, chiefly stone implements found in Brazil. . . . By Virgil von Helmreichen of Rio de Janeiro.

(2) On a collection of Peruvian antiquities in the Cabinet of Senhor Bartoza of Rio de Janeiro. By Thomas Ewbank.

Of course the Society could not afford to turn its attention away from the "diversity of languages, the remains of ancient art, and traces of ancient civilization in Mexico, Central America, and Peru." That would have done no good, and defeated the purposes of ethnology as well as archaeology. But there was also still less reason why Mexican and Peruvian researches should lie at the core of Indianist studies, while lowland and woodland peoples continued to roam, in several senses, far from highland population centers, and along the periphery of archaeology and ethnology.

Any start, naturally, offered some progress away from excessive stress on the "big three" zones. Considering the number and complexity of the different peoples and cultures of Latin America, a surprisingly good beginning was made in the form of articles and reports about Nicaragua, Colombia, the Caribbean, and Brazil. It was E. G. Squier, much more of a scholar than Norman or Stephens, who revealed the possibilities of Nicaragua in his article entitled, *"Archaeology and Ethnology of Nicaragua,"* (Trans. Am. Ethnol. Soc. 3: 86-158). For the first time, American ethnologists had field information about customs, native industries, house architecture, language, and environment.

Besides nominating members from Colombia and Cuba, the Ethnological Society opened its pages to descriptions of the state of research in Colombia. Its *Proceedings* printed—for the first time in the United States—an account of the travels and observations of the noted New Granadan scholar, José María Vergara y Vergara. The Cuban corresponding member of the Lyceum of Natural History and the Ethnological Society, Andrés Poey, introduced to Americans the theme of "Cuban Antiquities." The rest of the Caribbean-Central American zone was surveyed, if not analyzed, by Berthold Seeman in his account of the *"Aborigines of the Isthmus of Panama."*

While it may seem from this that interest in the Caribbean and Central America lay unnecessarily close to regions of current economic and political value, there were real advantages in ethnological expansion. Any step away from Mexico and Yucatan inevitably had to lead to countries geographically contiguous, and it definitely advanced progress in Latin American studies. There were, as a result, wider possibilities of work and an attempt to bring about the "decentralization" of American Indian studies. The trend was remarkable not so much for the knowledge which grew as for new interests, contacts, and inter-American memberships it created.

More and more of Latin America came into the scope of the Ethnological Society's work. The Society was quite conscious of the great gap in knowledge in spite of conspicuous discoveries in Middle America. There remained a vast area of importance, described largely by European travelers. This new domain of ethnology was Brazil, and the Society began to turn its attention to Brazilian Indian studies. Here again, Bartlett, acting under the instructions of the Ethnological Society, and working together with the New York Historical Society, used his offices to bring about the first concerted and organized effort to make Brazilian ethnology better known in the United States. These proposals antedate by twenty years the expeditionary reports made by Agassiz and Hartt, and Bartlett should emerge from an ill-deserved anonymity for having opened direct exchange of publications and membership with the noted *Instituto Geographico e Historico Brasileiro* in Rio de Janeiro.

This year of decision and contact, 1846, although a year of war, did not stop the work of cultural cooperation. The New York Historical Society, already interested in Spain and Latin American archaeology and ethnology, encouraged the Ethnological Society's initial steps towards Brazilian friendships. It is true that, before this, individual Brazilians had been brought by the American Philosophical Society into the circle of inter-American corresponding membership, and this pattern of scholarly links continued with the Lyceum of Natural History, the Academy of Natural Sciences in Philadelphia, and the American Geographical Society. But it was still a little rare for American societies to seek to tie their work permanently and purposely to inter-American scientific cooperation. Certainly, such ties were new for ethnology in America.

As secretary of both the Ethnological and the Historical Society, Bartlett steered the interests of both towards an open door of cultural policy through which many noted Americans were to pass in the next hundred years. Intellectual reciprocity took on the outlines of its present shape when Bartlett introduced the work of American ethnology to Brazil. The *Revista Trimensal* of the *Instituto Geographico e Historico* (8: 153) described the cultural overtures:

> "146th Meeting, March 18, 1846.
>
> "Letter from Mr. John Russell Bartlett, secretary of the American Ethnological Society, in New York, inviting this Institute, with the compliments of that Society, to initiate a fraternal and literary correspondence, which will surely be of no small benefit to both societies, and in general to the promotion of science on the American continent. Together with his letter, Mr. Bartlett sends, for the library of the Institute, the first volume of the *Transactions* of the Ethnological Society, along with the following works offered by their authors."

This exchange, soon to be followed by the *Instituto's* own shipment to the Ethnological and other Societies, was the first of a long series of gifts and exchange of publications between Brazilian and United States groups.

The books which Bartlett sent down were those which he considered to be good specimens of North American work. They include:

(1) Alexander W. Bradford. *American Antiquities and Researches into the Origin and History of the Red Race.* New York: 1841.

(2) Samuel George Morton. *An Inquiry into the Distinctive Characteristics of the Aboriginal Race of America.* Philadelphia: 1844.

(3) Samuel George Morton. *A Memoir of William Maclure Esq., late President of the Academy of Natural Sciences of Philadelphia.* Philadelphia: 1844.

(4) William H. Hodgson. *Notes on Northern Africa, the Sahara, and Soudan in Relation to the Ethnography, Languages, History, Political and Social Condition of the Nations of those Countries.* New York: 1844.

(5) Benjamin M. Norman. *Rambles in Yucatan; or Notes of Travel through the Peninsula Including a Visit to the Remarkable Ruins of Chichen, Kabah, Zayi, and Uxmal.* New York: 1844.

At the March meeting of the *Instituto,* corresponding membership was extended to all the North American authors cited, and, on its own initiative, the *Instituto* sent to New York City for other copies of these works and a copy of John Stephen's *Incidents of Travel in Central America, Chiapas and Yucatan.* A final touch was given by beginning correspondence with the American Ethnological Society, and Albert Gallatin, its president, was made an honorary member of the Brazilian Society, while John Bartlett, secretary, was named as corresponding member of the Rio de Janeiro group.

In this way, by the beginnings both of interest and of research in continental America, the search for knowledge regarding Indian society led to inter-American ties with Mexico, Cuba, Colombia, Central America and, ultimately, Brazil. The horizons were now extended—even hemispheric—and large plans for consolidating cultural relations, to the point of calling inter-American ethnological congresses, were being advanced. While the war with Mexico, then going on, won few friends in Latin America, the pace and purpose of inter-American ethnological relations continued and, indeed, brought forth an invitation to Brazilian scholarship which constitutes enough of a landmark to be partially repeated here. The cited letter came before the whole membership of the Brazilian Society on October 29, 1846. It was preceded by the following which renewed the request for cultural relations:

> "Letter from our corresponding member, Mr. John R. Bartlett, Secretary of the New-York

> Historical Society, offering the *Instituto* in the name of that Society, the collection of its transactions during the year 1845, and declaring that it would greatly prize a correspondence with this Instituto, whose publications it will always receive with great satisfaction. . . .
>
> "Another from the same gentleman, in his capacity as Secretary of the American Ethnological Society, in whose name he offers to the Instituto the first volume of its Transactions. He similarly expresses the desire of the Ethnological Society to enter into a fraternal correspondence with the Instituto Brasileiro, reciprocating the exchange of publications."

But Bartlett and the Ethnological Society had plans of longer range which would place this type of inter-American cooperation on a permanent footing of unity. He included in his correspondence a longer note, sent by his colleague Herman Ludewig of the New York Historical Society and dated, New York, August 25, 1846. The note was read to the membership of the Brazilian *Instituto,* and only the most important excerpts follow:

> "Gentlemen:
>
> . . ."A congress of all the historical societies of the American continent towards this important end [the history of the New World] could not fail to have the most happy consequences. Even if these historical societies only communicate reciprocally the results of their respective investigations we will already have advanced along one third of the way. Up to the present no one has paid attention to this, and Europe continues to centralise toward itself the efforts of even the most national-minded scholars of the New World. . . .
>
> "May not the students of New World History hope that a brotherly and scientific association, that a mutual exchange of each other's publications, will soon come to replace this [aloofness]? That North and South America can assist one another's researches, whose results from now on be useful to both? That all Americans, finally, will make common cause in exploration of the historical treasures of their vast continent?
>
> "The immense advantages of association supply us with a guarantee that these hopes, these desires are not far from fulfillment, and it is because of this belief that the New-York Historical Society and the American Ethnological Society are the first to extend a friendly hand to you in close scientific and fraternal relations. . . ."

The unity of history and ethnology in the Americas would have been the cornerstone of this Congress, had such an assembly ever come into being. Up to then, the connection between history, ethnology, and geography in Latin American research was a pretty close one. The method common to each was to approach the problem through literary sources, while interpretation was influenced by a mixture of romanticism, a universalist ideal, and the search for continental unity. As long as there was a shared interest in Indian origins and the aboriginal past, history in the 19th century was a blood brother of ethnology. However, history and ethnology fell away from this relationship due, partly, to the advancement of field method, as well as to the rise of nationalist historians in Latin America who were concerned with European institutions, laws, and government in America.

The elements of inter-American relations continued to be New World man, geography, and science, and it fell to anthropological science to try to re-establish at least an intellectual kinship in the New World. This was a hard task, no matter how much the learned and scientific societies had fastened the ties of inter-American cooperation. As much as individuals and societies stimulated the rise of cultural relations, the cost of exchange and the expenses of shipments were a financial strain. Nevertheless, the societies tried to retain as long as they could the advantages of direct exchange and communication with Latin American science. It is at this moment that the Smithsonian Institution entered upon the established cultural routes to Latin America, promoting inter-American scholarly exchange on a wide and highly reputable scale.

Although no inter-American congress of the learned societies of North and Latin America ever came about, still the Smithsonian, by following the exchange precedents of the Ethnological, the Philosophical, New York Historical and many other Societies, supported the aims of private learned groups with the assistance of government and gave new scope to ethnology and archaeology in the Americas. The record of inter-American relations as a normal part of American science owes a great deal to the individual societies whose work has been described, but as much and more is owed to the Smithsonian Institution. The fact that the Smithsonian was created by Act of Congress did not impair relations in Latin America, because no other learned American group, unless it be the American Philosophical Society, has ever been as highly regarded by Latin American scholarship and science.

Since the ethnology of America was at that time a large part of the Smithsonian's interest, the trend of its efforts renewed the influence of philology and grammar. Moreover, Indianism continued to seek through language for evidence of unity of stock between English and Spanish North America. The Smithsonian, which did not create, but rather followed the accepted areas, patterns, and methods of study, took little cognizance of the ethnology of continental Latin America, or of Brazil. The importance of linguistics, therefore, attracted the Smithsonian to Mexican scholarship in this subject. But its *Contributions to Knowledge* and the *Transactions* were circulated widely in the hemisphere, and scholars and societies in Bogotá, Caracas, Santiago, Buenos Aires, and Rio de Janeiro knew of the work being accomplished by North American ethnologists.

It was in connection with the project on the *Handbook of the North American Indian,* and the preparation of linguistic maps of aboriginal Mexico, that Mexican ethnology entered into joint participation with the United States for the first time. The Smithsonian acted as clearing-house and sponsor. For example, in 1862 and 1863, the Smithsonian was directed by one of the most active proponents of inter-American relations, the celebrated physicist Joseph Henry. The excellent success of his efforts to bring the science of both Americas together is to be described elsewhere, but at this time he encouraged steps on behalf of ethnology. Not the least of his many helpful acts was to secure from A. J. de Irisarri, the Guatemalan poet and Minister to the United States, necessary letters of introduction for Dr. Behrendt's ethnological work in Yucatan and Central America. Behrendt, an assiduous correspondent of John Russell Bartlett, had received a grant from the Smithsonian to carry on investigations at Petén and Vera Paz.

But it was Mexican ethnology which continued to be the major lodestone and attraction. Dr. Henry, on behalf of the Smithsonian, could not stand aside from the Indianist riches of Mexico. As an object for study by archaeologists, historians, and ethnologists, that country was unparalleled in America. To advance knowledge about Mexico, it was necessary to go beyond the central valley civilization and find out something about the many other Indian languages, peoples, and tribes. Even some living ethnology had to be known. In this situation, the Smithsonian called upon Buckingham Smith, who knew Mexico, had studied in the Spanish archives, and had done some work on Indian philology in Northwestern Mexico.

Smith was in a position to suggest the name of his good friend, José Fernando Ramírez, as the most competent director of the Mexican part of the proposed *Handbook.* Ramírez, it may be added, was already well enough known in the United States to have been appointed a corresponding member of the American Ethnological Society, the New York Historical Society, and the American Antiquarian Society. It was with regard to this point that Professor Henry was addressed by George Gibbs, secretary of the New York Historical Society, whom we have already seen to be interested in

Yucatecan investigations. In writing about the *Handbook,* Gibbs's letter shows how far the inter-Americanism of linguistic ethnology had been developing:

> "January 21, 1863
>
> "Mr. Buckingham Smith called on me today and showed me a letter from Don José Fernando Ramirez, of which I enclose an abstract.
>
> 'There exist no vocabularies of the languages nor have the grammars ever been preserved, written by the early missionaries. It is almost impossible to bring together those that have been printed. On this subject a work has been commenced entitled *Cuadro Descriptivo y Comparativo de las Lenguas Indigenas de Mexico,* by Don Fernando [*sic* for Francisco] Pimentel. The first volume only has been printed which comprehends the analysis of twelve languages. Unfortunately material is wanting. Those contained in the first volume are the Huaxteco, Mixteco, Mame, Othomi, Mexican, Zapoteco, Tarahumar, Tarasco, Totonaco, Opata or Teguema, Cahila, and Matlaznica. If you have succeeded in publishing the grammars about which you informed me [Buckingham Smith published the Pima and Esleve grammars in collaboration with John Gilmary Shea], and they should arrive in time, they will be examined in the work.
>
> 'I have not seen and am unacquainted with the *Archaeology of the United States,* by Samuel F. Haven, about which you write me. Of the Smithsonian *Contributions* I have only the second, third, and fourth volumes, unless the first volume should be the *Ancient Monuments of the Mississippi Valley* [Squier] which I possess. At present there is no way of sending books to Mexico unless the Department of State will take charge of them.'
>
> "Mr. Smith has handed me the above with the view that I might ask of you to send to Señor Ramirez such other papers of the *Contributions* as belong to archaeology. That gentleman is well known as one of the most distinguished scholars in that department in Mexico, and one whom it would be desirable for the Institution to number among its correspondents. I am, however, astonished at the account he gives of the paucity of works on the indigenous languages of that country, so entirely opposite to our general belief here. Under any circumstances, Pimentel's work should be procured, if possible."

This single example is sufficient to indicate the entrance of the Smithsonian into inter-American ethnology. The Institution's aid was important, but it followed an already existing path. Not was this was unusual; in fact, very few had up to then thought to look to geology and paleontology in the rest of Latin America for understanding of American prehistory. The golden sun of Cuzco and Tenochtitlan threw deep shadow and eclipse on the rest of Latin America, in spite of several attempts to enter into research contact with that part of the continent. The word-riches of philology, likewise, were as bright as they were obscuring. Thus, it was long overlooked that the rest of America also had a past, not recorded in pictograph or glyph.

John Bartlett, for the American Ethnological Society, had forced some exceptions to this emphasis upon Mexico and Peru. In addition to the cited efforts to establish organizational ties, he encouraged correspondence—later published—from Virgil von Helmreichen of Rio de Janeiro, who had been among the Indians of the Serra do Espinhaço in the Rio Sao Francisco Valley and gave the Ethnological Society information (together with his own drawings) of artifacts, idols, rock paintings, shells, etc. Since the present-day Indians no longer made those objects, Helmreichen submitted the view (as Charles Hartt was to do for the Amazon) that an older civilization had once inhabited those places of Brazil.

But the intellectual father of effective North American scientific interest in Brazilian antiquities was Charles Frederick Hartt. Canadian by birth, but American in training, Hartt went on to do what Bartlett and Ludewig had outlined in their Brazilian correspondence twenty years before. Cut off by yellow fever at the age of 38, Hartt, originally a geologist serving under Agassiz, became the first trained American to become interested in Brazilian ethnology and Indian antiquities. He had combined geology with paleontology in the Amazon and Sao Francisco Valleys, and went on to separate the study of the Brazilian past from charlatans and superficial students. It was his feeling that in the Amazon and Rio Sao Francisco Valleys lay evidence of old earth strata, together with the vestiges of ancient culture. Far from the highland culture to the west, Brazilian human and natural history could be integrated through ethnology and paleontology.

Hartt's former students write of him that he

> "busied himself studying Munducurú and Maué dialects of the modern Tupí language of the Amazonas, and in bringing together for the first time the stories and myths which are current in the tribes. He [had] prepared a large volume on the grammar, vocabularies and stories of this language, which yet [1878] remains unpublished."

His grammar and dictionary of the Tupí (still unpublished) was given by his wife to the *Museu Nacional,* of which he was the first North American to have been Assistant Director. Of his other contributions, his manuscript notes, rich in observed field data among Amazon tribes and archaeological finds at Bahia and Pará, have not been published, nor his projects for study of Brazilian antiquities and mythology. Some of his works on Indian pottery, rock inscriptions, myth, and native cemeteries were published in the *American Naturalist,* the *Archivos do Museu Nacional* (Rio de Janeiro), or as slender booklets. His work on the Botocudo was reprinted in his best-known book, *Geology and Physical Geography of Brazil,* recently re-issued in Brazil.

Hartt was very well known and highly regarded in his lifetime both as a geologist and an ethnologist. It is strange that his manuscript materials have never been published. As a geologist, he trained several Americans who won fame for their Brazilian work, one of whom took over his post in Brazil. But as an ethnologist his death cut off an effort which would have increased knowledge, and his eyewitness and field studies of the Indians, presumably on file in the *Museu Nacional,* were never used by others.

Fortunately, his unique position with the Brazilian government, his office in the *Museu Nacional,* and his own contributions gave him the reputation which won for him financial aid from the Peabody Museum of American Archaeology and Ethnology. By this time (1870), the Peabody Museum had begun to support and aid research in Latin America, having given a grant of $352 to Dr. Behrendt for Central American and Yucatecan ethnology (1860), and to Porter C. Bliss, corresponding member of the Mexican *Sociedad de Geografía y Estadística,* the sum of $250 for research in Mexico during 1871. The directors of the Peabody Museum did not hesitate to grant Professor Hartt $500 at one time (1872), and $600 in 1875 for his Brazilian studies. The Peabody grants to Hartt must be considered as the first grant-in-aid for anthropological field work in Brazil, and as another long step towards North American recognition of Brazilian ethnology.

Hartt, who had worked very closely with Brazilian colleagues in geography and ethnology, and who recommended these associates to membership in United States scientific societies, did a great deal of work to establish the validity of the single origin of the American Indian. American Indian civilization was not polygenous—as his former chief, Agassiz, thought—but was a product of American soil. This thesis, championed vigorously from the earliest days of inter-American interest, now rested for the first time upon the additional proof of geology and paleontology, whereas its earlier support had been largely literary and speculative. And Hartt, in keeping with the idea of progress, also held that Botocudo skulls, however primitive the ethnology of that people, did not represent any degeneracy from the assumed *Urmensch* of the New World.

It was in his report on *"Ancient Indian Pottery of Marajó, Brazil,"* that he made his position clear. He had found his pottery and mortuary urns lying in mounds on Marajó Island in the Amazon. His summary follows:

> "We have no historical records of the tribe that built the Marajó mounds. . . . We have no record of the existence of any tribe in the Lower Amazonas that buried its dead in jars. I do not feel like coinciding with von Martius in the supposition that the Marajó mounds were made by Indians of Tupí descent. There are many resemblances between the pottery of Marajó and that of Peru and North America that will be worth study. I hope that future explorations will enable me to clear up much more of the doubts expressed in this paper and cast much needed light on the ancient races of the Amazonian Valley."

In separate papers and articles, Hartt added more certainty to this opinion. His study of the rock inscriptions on the Serra do Ereré on the Tocantins, and those of the Rio Sao Francisco were checked against literary descriptions made in the 18th century. He became more and more convinced that the Tupí had displaced an earlier people, probably were civilized. The Tupinambá succeeded the people of mounds, burial urns, and rock markings.

Hartt wrote several articles which he hoped would bring about more public and scientific interest in Brazilian ethnology. Considering what Bartlett and the Ethnological Society had tried to begin years before, it is interesting to see how Hartt repeated the problem (quoted from *American Naturalist*, 1871):

> "It is a great shame that the antiquities of Brazil have so far received little or no attention, yet the country is one whose ethnology is extremely interesting and it is very desirable that the history of its many tribes should be traced out. The neglect of Brazilian antiquities has arisen, no doubt, from the comparative rarity of the relics and the difficulty of exploring the country. Stone implements are found all over the Empire, ancient pottery occurs in many localities, especially in burial stations, and Kjökkenmöddings [sambaquies] exist on the coast as at Santa Cruz in the Province of Espiritu Santo, on the Bay of Rio de Janeiro, at Santos, and elsewhere."

The initiative in this work, however, rested upon only one individual, and no American ethnologists came in to take over from where he had left off. It took a long time before the empty spaces in Brazilian antiquities were filled in by Americans. The reasons for this lie in the situation back home in the United States, where interest was directed to the Southwestern and Western Indians, Mexican archaeology, and the ever-important study of linguistics.

Years of effort and labor in the field of Indian grammar and language were now ripened by the study of native myth, legend, and literature in connection with aboriginal civilization. At the University of Pennsylvania, a Chair in American Archaeology and Antiquities supplemented the name and aim of the Peabody Museum at Harvard. The universities of North America took up the problem which scientific and scholarly societies had opened to inquiry and started to pursue. The Chair at Pennsylvania was held by Daniel G. Brinton, whose interest in myth, literature, and linguistics was almost exclusive with him. Brinton attempted to synthesize the grammatical and philological systems of aboriginal America into a literary picture which would describe the history of the civilization. Again, language and literature went hand in hand as main factors in Indian history.

Brinton's emphasis on myth—although he lacked the social and psychological insight brought to this approach today—brought on him criticism for ignoring field ethnology and relying too heavily on books and their writers. His views, in spite of this, were strong and certain, probably because he was well read in the language of the Spanish and native authors on Mexico. Although he drew up linguistic maps of the Chaco and other South American groupings, his main concern was with Mexican and Central American civilization.

But by this time, American ethnology had other workers who were not limited to bookish accounts and library opinions, who traveled widely and resided in northern as well as southern Spanish America, although not in Brazil. Adolphe

Bandelier was such a field worker, yet he was just as much at home in the Spanish literary sources dealing with aboriginal culture and, especially, native society. He held strong opinions on the damaging influence of Brinton upon American Indian studies. What he had to say can be summed up as the criticism of a century of ethnological method:

> "Santa Fé, New Mexico, January 1885. . . ."what Dr Brinton has to say . . . has not much to signify. He is a very worthy, active, and industrious man, but only a reader and writer. American aboriginal history has never entered into his flesh and bone through practical experience. So it is the case with many, they gaze at things from an easy-chair, cite the nation before them into an elegant study, and when he is seated (after they have washed, combed, and perfumed him), they question him, carefully noting what he has to say. In this manner they study Ethnology! Archaeology is gathered from specimens only, and not a moment's attention is devoted to the condition under which they are found as well as to their relations to the country where they have been used. . . ."

While the method and conclusions of Bandelier are themselves open to criticism, this is not the place for their discussion. By the end of the century, and after, the technique of ethnological approach was to be made somewhat clearer and more certain.

The historical fact is that interest in Latin American Indianism had come to be permanent in the United States, after a century of growth and ripening of method. In a certain sense, anthropological pan-Americanism is probably the oldest form of our intellectual interest in Latin America. Curiosity about the origins and nature of the Latin American Indian brought it into being during the 18th century as part of the common setting shared by both Americas, but it was an active search for contact with the minds and societies of Latin America in the 19th century which gave it direction, organization, and inter-American unity. The matter of method, and the clarification of ideas of approach to Indian culture as a technique, never stood in the way of the hemispheric association of ethnologists. Whether emphasis lay upon grammar and linguistics, archaeology, or field ethnology, the joints of mutual relations held the cooperative structure together. Anthropology, history, and archaeology, in probing far back into the past for the possible unity of man in America, thus contributed considerably to the cultural unity of the present.

SECTION OF PHYSICS AND CHEMISTRY

DIELECTRICS AND RHEOLOGY OF NON-AQUEOUS SUSPENSIONS

By ANDRIES VOET*

The microscopic and electron microscopic methods of investigation of suspensions of finely divided materials in liquid media have not given a deeper insight into the mechanism of particle agglomeration or deflocculation, primarily due to unavoidable differences between observed preparations and actual concentrated suspensions.

The rheological behavior of concentrated suspensions, often characterized by plasticity and thixotropy, made it clear that, generally, particle structures were formed or broken up upon variation of the shearing stresses to which the systems were subjected. A proper interpretation of rheological data at low shearing stresses, however, remains doubtful, due to the phenomenon of plug flow, while nothing at all can be said of the rebuilding of previously destroyed particle structures at complete rest.

The present investigation is characterized by the simultaneous measurement of dielectrical and rheological properties of suspensions at lower as well as at higher shearing stresses and by the measurement of the progressive dielectric changes occurring in the systems observed. Experiments are reported with dispersions of particles having a relatively high dielectric constant, such as metals, carbon black, titanium dioxide, etc. These systems were studied first, since the effects found were numerically larger and afforded a simplified mathematical treatment.

It was found that the dielectric constant is lowest for spherical particles. From dielectric data, a form factor could be established which proved to be a constant for each pigment, indicating the degree of deviation of the particle shape from the spherical form. In addition, an agglomeration factor was introduced which was indicative of the degree of agglomeration of particles at each concentration and at each shearing stress. By measuring dielectric changes in dispersions at rest, the progressive change in formation of particle chains could be followed.

The dielectric method of investigation appears to be more sensitive than the rheological one, and changes in particle structure which were completed in less than one second could be observed.

As a result of a number of experiments, it was established that plastic systems are all thixotropic, although the time factor is often too short to be detected rheologically. In addition, systems which show newtonian flow are characterized by the absence of particle agglomeration. Finally, it was found that particles which differ very markedly from the spherical shape, such as leaflets of metals, needles, etc., orientate under shear. The time factor involved in the changes from random distribution to the state of orientation of these particles, and *vice versa,* gives rise to orientation thixotropy.

The quality of a vehicle, to promote the dispersion of a specific solid, often called "wetting and dispersing characteristics," appears to be the power to prevent particle agglomeration. It may be quantitatively estimated as the highest concentration of the dispersed phase at which only single particles exist. Its value may be found from dielectric data at various concentrations and shearing stresses.

In the study of the phenomenon of thixotropy, the dielectric method appeared to be of importance. The so-called "false body" type of thixotropy, characterized by a comparatively short time factor and by large dielectric changes, appeared to be due to particle agglomeration.

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*J. M. Huber Corporation, Brooklyn, N. Y. This paper, illustrated by lantern slides, was presented at the meeting of the Section on October 24, 1947.

Another type of thixotropy, characterized by a long time factor and by large changes in consistency, was found to exhibit only slight dielectric changes. Its thixotropy must be ascribed to the formation of a "tactoid" structure, in which particles are immobilized in a vehicle as a result of minima of potential energy at a distance, without forming actual contact.

There are a number of practical applications which make the dielectric method a valuable analytical tool. Thus, a carbon black of any type can be easily identified by its form and agglomeration factors in a specific vehicle at a specific concentration. Since the agglomeration factor is indicative of the forces acting between the particles, its value is of importance for the carbon black characteristics in any liquid or solid dispersing medium. A very considerable variation for different types of black has been found.

The concentration of a known carbon black may be estimated accurately in a few minutes, in a dispersion of a vehicle of known dielectric constant—a procedure consuming time and inaccurate by conventional analysis. The dielectric method distinguishes immediately between densified and other types of the same carbon black, which is rather difficult to carry out by other methods.

Finally, the degree of dispersion of incompletely dispersed blacks may be found quantitatively from dielectric data.

The method of measuring dielectric changes simultaneously with rheological data has its application in other fields, such as, for instance, with high polymeric or high-molecular liquids or solutions of high-molecular substances. As an example, an investigation was made of the so-called thixotropic phenomena observed with heavy hydrocarbon oils at high shearing stresses, reported several years ago and ascribed to orientation of the particles of the liquid at very high shearing stresses.

Since orientation of molecules will give rise to a decreased dielectric constant, various heavy hydrocarbon oils were studied at very high shearing stresses, up to 2.10^5 dynes per cm.[2] A decrease in dielectric constant of several per cent was observed, indicating that orientation actually occurs. These changes, however, are instantaneous and reach their final value within a fraction of a second, for increasing as well as for decreasing rates of shear. Consequently, the actually observed gradual decrease of the shearing force at high rates of shear over a period of several minutes cannot be the result of particle orientation, but must be ascribed to other causes, most likely heat effects.

With bodied vegetable oils, on the other hand, a definite larger change and a distinct time factor extending over a longer period of time have been observed in dielectric data, indicating a true thixotropy.

CONFERENCES HELD

SECTION OF BIOLOGY

CONFERENCE ON "ANTIHISTAMINE AGENTS IN ALLERGY": OCTOBER 3 AND 4, 1947

The Section of Biology held a Conference on "Antihistamine Agents in Allergy." Doctor Fredrick F. Yonkman, Ciba Pharmaceutical Products, Inc., Summit, N. J., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, October 3

Morning Session. "Opening Address," by the Conference Chairman.

"The Pharmacology of Histamine," by Sir Henry H. Dale, formerly Director of the National Institute for Medical Research, Mount Vernon House, N.W.3, England.

"The Role of Histamine and Other Metabolites in Anaphylaxis," by Carl A. Dragstedt, Northwestern University Medical School, Chicago, Illinois.

"The Role Played by Leucocytes and Platelets in Anaphylactic and Peptone Shock," by M. Rocha e Silva, Instituto Biologico, Sao Paulo, Brazil.

Afternoon Session. Chairman, Robert A. Cooke, Roosevelt Hospital, New York, N. Y.

"The Immunologic Aspects of Anaphylaxis and Allergy," by M. W. Chase, Rockefeller Institute for Medical Research, New York, N. Y.

"The Detoxification of Histamine," by E. T. Waters, Banting and Best Department of Medical Research and Department of Physiology, University of Toronto, Toronto, Canada.

"Histamine-Azo Proteins in Anaphylaxis and Allergy," by Norbert Fell, Camp Detrick, Frederick, Maryland.

Saturday, October 4

Morning Session. Chairman, George Rieveschl, Jr., Parke, Davis and Company, Detroit, Michigan.

"Introduction of Antihistamine Agents and Antergan Derivatives," by Daniel Bovet, Instituto Superiore di Sanità, Rome, Italy.

"The Anti-Allergic Activity of Pyribenzamine and Related Compounds, with Special Reference to Their Mode of Action," by Rudolf L. Mayer, Ciba Pharmaceutical Products, Inc., Summit, N. J.

"Specificity of Antihistamine Drugs," by Earl R. Loew, Department of Pharmacology, University of Illinois College of Medicine, Chicago, Illinois.

Afternoon Session. Chairman, Sanford B. Hooker, Boston University, School of Medicine, Boston, Massachusetts.

"Pharmacological Studies of the Antihistaminic Thephorin, by G. Lehmann, Hoffmann-La Roche, Inc., Nutley, N. J.

"Antistine and Related Imidazolines," by Rolf Meier, Medical Director, Ciba Limited, and University of Basel, Basel, Switzerland.

"The Mode of Action of Antihistaminic Agents in the Skin," by Charles F. Code, John U. Keating, and Milo D. Leavitt, Mayo Foundation, Rochester, Minnesota.

"Antihistamine Therapy, Experimental and Clinical Correlation," by Samuel M. Feinberg, Northwestern University Medical School, Chicago, Illinois.

CONFERENCE ON "THE CHEMOTHERAPY OF FILARIASIS": OCTOBER 17 AND 18, 1947

The Section of Biology held a Conference on "The Chemotherapy of Filariasis." Doctor J. H. Williams, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, October 17

Morning Session. Chairman, Arnold D. Welch, Western Reserve University, School of Medicine, Cleveland, Ohio.

"Introductory Paper," by L. T. Coggeshall, University of Chicago, Medical School, Chicago, Illinois.

"General Experimental Methods Used for Studying Filaricides," by Redginal I. Hewitt, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

"Use of Aresenicals in Filariasis," by G. F. Otto, School of Hygiene and Public Health, Johns Hopkins University, Baltimore, Maryland.

"Use of Antimonials in Filariasis I. Anthiomaline," by R. W. Brown, School of Public Health of the Faculty of Medicine, Columbia University, New York, N. Y.

"Use of Antimonials in Filariasis II. Neostibosan and Some Other Compounds," by J. T. Culbertson, University of Arkansas School of Medicine, Little Rock, Arkansas.

"Tolerance of Antimony and Arsenic by Intensively Treated Patients," by H. M. Rose, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Afternoon Session. Chairman, J. H. Williams.

"Use of Cyanine Dyes in Filariasis I. Chemistry of the Cyanine Dyes," by L. G. S. Brooker, Eastman Kodak Research Laboratories, Rochester, N. Y.

"Use of Cyanine Dyes in Filariasis II. Chemotherapeutic Activity in the Cotton Rat," by H. N. Wright, J. T. Litchfield, Jr., T. Brey, E. M. Cranston, A. C. Cuckler, and R. N. Bieter, University of Minnesota Medical School, Minneapolis, Minnesota.

"Use of Cyanine Dyes in Filariasis III. Effect of Cyanine Dyes on the Metabolism of *L. Carinii*," by Ernest Bueding, Western Reserve University, School of Medicine, Cleveland, Ohio.

"Use of Cyanine Dyes in Filariasis IV. Pharmacology," by L. Peters, Western Reserve University, School of Medicine, Cleveland, Ohio.

Saturday, October 18

Morning Session. Chairman, L. T. Coggeshall.

"Use of Piperazines in Filariasis I. Chemistry," by Samuel Kushner, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

"Use of Piperazines in Filariasis II. Parasitology," by Redginal I. Hewitt, D. E. White, S. Kushner, W. S. Wallace, H. W. Stewart, and Y. SubbaRow, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

"Use of Piperazines in Filariasis III," by B. K. Harned, Raymond W. Cunningham, Sybella Halliday, R. E. Vessey, N. N. Yuda, Mary C. Clark, and Y. SubbaRow, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

"Clinical Report on the Treatment of Filariasis I. Parasitology," by J. Oliver-Gonzalez, School of Tropical Medicine, San Juan, Puerto Rico.

"Clinical Report on the Treatment of Filariasis II. Clinical," by D. Santiago-Stevenson, School of Tropical Medicine, San Juan, Puerto Rico.

CONFERENCE ON "MEDICAL MYCOLOGY": OCTOBER 31 AND NOVEMBER 1, 1947

The Section of Biology held a Conference on "Medical Mycology." Doctor Frederick Reiss, New York University, Medical Colege, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, October 31

Morning Session: General Mycology. Chairman, Carroll W. Dodge, Missouri Botanical Garden, St. Louis, Missouri.

"Introductory Paper," by the Conference Chairman.

"Mycological Research and the Progress of Medicine," by Carroll W. Dodge.

"Public Health Aspects of Fungus Infections," by S. B. Salvin, National Institute of Health, Bethesda, Maryland.

"Evaluation of Classification of Pathogenic Fungi," by Morris Moore, The Barnard Free Skin and Cancer Hospital, St. Louis, Missouri.

"Future Developments in Mycological Investigative Methods," by Norman F. Conant, Duke University, School of Medicine, Durham, North Carolina.

Afternoon Session: Special Clinical Mycology. Chairman, Arturo L. Carrión, School of Tropical Medicine, San Juan, Puerto Rico.

Exhibition and Demonstrations by E. Muskatblit, New York University, Medical College, New York, N. Y.

"Superficial Dermatomycoses Caused by Trichophytons, Microsporons and Epidermophytons," by Fred D. Weidmann, University of Pennsylvania, Philadelphia, Pa.

"Chromoblastomycosis," by Arturo L. Carrión.

"Histoplasmosis," by Amos Christie, Vanderbilt University, School of Medicine, Nashville, Tennessee.

"Cryptococcosis and Blastomycosis," by Rhoda W. Benham, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Evening Session. Chairman, Norman F. Conant.

Exhibition and Demonstrations by E. Muskatblit.

"The Nutritional Requirements of the Faviform Trichophytons," by Lucille Georg, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"New Insight Gained in General Pathology and Practical Medicine by the Study of Sporotrichosis," by H. Gougerot, Hôpital Saint-Louis, Paris, France.

Saturday, November 1

Morning Session: Physiology, Immunology, and Epidemiology. Chairman, William J. Robbins, The New York Botanical Garden, New York, N. Y.

"The Growth Requirements of the Dermatophytes," by William J. Robbins.

"Fungus Antigens and Their Importance as Sensitizers in the General Population," by Samuel M. Peck, The Mount Sinai Hospital, New York, N. Y.

"Practical Applications of Immunological Principles in the Diagnosis and Treatment of Fungus Infections," by Donald C. Martin, Duke University, School of Medicine, Durham, North Carolina.

"Histoplasmin Skin Test," by Alexander M. Iams, Minneapolis, Minnesota.

"Some Biochemical Implications from a Study of Growth of Pathogenic Fungi on Media Containing Single Amino Acids," by R. M. Archibald, The Johns Hopkins University, Baltimore, Maryland, and Frederick Reiss, New York University, Medical College, New York, N. Y.

"Discussion of the Recent Trends in the Therapy of Superficial Fungus Infections," opened by Marion B. Sulzberger, New York Post-Graduate Medical School, Columbia University, New York, N. Y.

NEW MEMBERS

A. Elected from May 15, 1947, to September 15, 1947

LIFE MEMBERSHIP

Sanders, Murray, M.D., Biology, Physics, Chemistry. Associate Professor of Bacteriology, Coll. of Phys. and Surgs., Columbia University, New York, N. Y.

Saslow, Herbert Basil, M.D., Reproduction Physiology, Genetics. Teaching Assistant, University of Illinois, Urbana, Ill.

Wasserman, Louis Robert, M.D., Biophysics. Adjunct Physician for Physiological Hematology, Mt. Sinai Hospital, New York, N. Y.

SUSTAINING MEMBERSHIP

Gallenkamp, Charles Benton. Assistant Curator, Departments of Anthropology and Paleontology, American Museum of Natural History, New York, N. Y.

Geary, John R., Jr., M.D., Respiratory Physiology. Department of Physiology, Coll. of Phys. and Surgs., Columbia University, New York, N. Y.

Henze, Carlo, M.D., Internal Medicine, Pharmacology. Medical Director, Pharmaceutical Division, Sandoz Chemical Works, Inc., New York, N. Y.

Insell, Benjamin B., D.D.S., Industrial Chemistry and Physics. New York, N. Y.

Levy, David M., M.D., Psychodynamics. Assistant Professor of Psychiatry, Columbia University, New York, N. Y.

Lyman, Richard Sherman, M.D. Professor of Neuropsychiatry, Chairman of Department, Duke Hospital, Durham, N. C.

Manginelli, Piero, M.D., Biology. Assistant in Medicine, Department of Histology, Hospital Das Clinicas, Sao Paulo, Brazil.

Marshak, Richard H., M.D., Gastro-Enterology, Radiology. Assistant Radiologist, Mt. Sinai Hospital, New York, N. Y.

Proskauer, Richard, B.S., Physics, Servo Controls, Gyroscopics. Project Engineer, Sperry Gyroscope Co., Great Neck, L. I., N. Y.

ACTIVE MEMBERSHIP

Abreu, Benedict E., Ph.D. Assistant Professor of Pharmacology, University of California Medical School, San Francisco, Calif.

Adler, Edward, Ph.D., Chemistry, Electrochemistry. Industrial Consultant, New York, N. Y.

Agren, Gunnar, M.D. Associate Professor of Medical Chemistry, University of Uppsala, Uppsala, Sweden.

Allen, Thomas H., Ph.D. Instructor, Department of Physiology, Coll. of Phys. and Surgs., Columbia University, New York, N. Y.

Allman, David Bacharach, M.D., F.A.C.S., F.I.C.S. Senior Surgical Chief, Atlantic City Hospital; Surgical Chief, Betty Bacharach Home for Afflicted Children; Consulting Surgeon, Atlantic County Hospital for Diseases to the Chest; Surgeon, Atlantic County Hospital for Mental Diseases, Atlantic City, N. J.

Altschul, Frank Joseph, M.D., Medicine, Psychology. Director, Medical Service, Monmouth Memorial Hospital, Long Branch, N. J.

Applbaum, Karl, J.S.D., Psychology, Anthropology. Brooklyn, N. Y.

Aries, Robert S., D.Ch.E., Cellulose, Lignin, High Polymers. Research Associate, Polytechnic Institute, Brooklyn, N. Y.

Asper, Samuel Phillips, Jr., M.D., Internal Medicine, Endocrinology. Instructor in Medicine, Johns Hopkins Medical School, Baltimore, Md.

Bader, Mortimer Eugene, M.D., Physiology, Biochemistry. 1st Lieutenant, Medical Corps., U. S. A., Climatic Research Laboratory, Lawrence, Mass.

Bader, Richard Arthur, M.D., Medicine, Allied Sciences. 1st Lieutenant, Medical Corps., U. S. A., Climatic Research Laboratory, Lawrence, Mass.

Baehr, George, M.D., Internal Medicine. Chief of 1st Med. Service and Director of Clinical Research, Mt. Sinai Hospital; Clinical Prof. of Medicine, Columbia University, New York, N. Y.

Baez, Silvio, M.D., Physiology. Research Fellow, Cornell Medical College, New York, N. Y.

Bauman, William C., Ph.D. Research Chemist, Dow Chemical Company, Midland, Mich.

Baylor, Edward R., M.S., General Physiology. Teaching Assistant, Princeton University, Princeton, N. J.

Baylor, Martha Barnes, Ph.D., Biology. Senior National Research Council Fellow, Rockefeller Institute for Medical Research, Princeton, N. J.

Bear, Richard S., Ph.D., X-Ray Diffraction Studies of Tissues and Natural Polymers. Professor, Department of Biology, Massachusetts Institute of Technology, Cambridge, Mass.

Benesch, Reinhold, M.Sc., Chemical Pathology, Biochemistry, Nutrition. Instructor, Department of Biochemistry, School of Hygiene, Johns Hopkins University, Baltimore, Md.

Benjamin, James W., Ph.D. Professor of Anatomy. New York Medical College, New York, N. Y.

Berend, Gertrude, Ph.D. Literature Chemist, Hoffman-LaRoche, Inc., Nutley, N. J.

Berkman, James, M.D., Medicine. Fellow in Pathology, Montefiore Hospital, New York, N. Y.

Berman, Rose L., B.A. Biologist, Food Research Laboratories, Long Island City, L. I., N. Y.

Bernard, Jack, M.A. Psychologist, Klein Institute for Aptitude Testing, New York, N. Y.

Bernstein, Harry, Ph.D., Latin American Anthropology. Instructor, Department of History, Brooklyn College, Brooklyn, N. Y.

Bernstein, Jeane, M.S., Biology, Zoology. Laboratory Instructor, Brooklyn College, Brooklyn, N. Y.

Bevans, Margaret, M.D., Pathology, Experimental Arteriosclerosis. Experimental Pathologist, Goldwater Memorial Hospital, Welfare Island; Assistant Professor of Pathology, Coll. of Phys. and Surgs., Columbia University, New York, N. Y.

Black, Herman, B.A. Teaching Fellow, Department of Biology, Washington Square College, New York University, New York, N Y

Blake, Clara Oltmann, M.A. Instructor, Department of Biology, Brooklyn College, Brooklyn, N. Y.

Blondheim, Solomon Hillel, M.D., Liver Disease. Pathology Appointment, Montefiore Hospital, New York, N. Y.

Blum, Lester, M.D. Adjunct Surgeon, Mt. Sinai Hospital, New York, N. Y.

Blum, Lucille Hollander, M.A., Child Development. Assistant, Guidance Laboratory, Teachers College, Columbia University, New York, N. Y.

Boell, Edgar J., Ph.D., Developmental Physiology, Experimental Embryology. Prof. of Biology, Osborn Zoological Laboratory, Yale University, New Haven, Conn.

Borison, Herbert L., M.S. Graduate Student, Teaching Assistant in Department of Physiology, Columbia University, New York, N. Y.

Bornor, Donald Humphrey, B.S., Proteins, Amino Acids. Supervisor of Research and Development on Industrial and Pharmaceutical Products, Sheffield Farms Co., Inc., New York, N. Y.

Bowes, Anna De Planter, M.A. Chief, Division of Nutrition, Pennsylvania Department of Health; Lecturer on Nutrition, School of Education, University of Pennsylvania, Philadelphia, Pa.

Bradley, E. Morton, Ph.D. Associate Professor of Anatomy, Temple University School of Medicine, Philadelphia, Pa.

Braley, Alson E., M.D., Biology. Assistant Professor of Ophthalmology, Columbia University, New York, N. Y.

Brink, Norman G., Ph.D. Research Chemist, Merck and Co., Inc., Rahway, N. J.

Brogan, Edythe Rose, B.A. Bacteriologist, Burroughs Wellcome Research Laboratories, Tuckahoe, N. Y.

Bryant, Gordon Hodges, Ph.D., Biochemistry, Biology, Psychology. Chairman of the Board, Frozen Food Institute, Inc., New York, N. Y.

Buchwalter, Francis, Ph.D., Physical Chemistry, Biology. Chief, Division of Physical Organic Research, Applied Sugar Laboratories, Inc., Brooklyn, N. Y.

Buno, Washington, M.D. Professor and Head, Department of Histology and Embryology, School of Medicine, University of Montevideo, Montevideo, Uruguay.

Burg, Marion, Ph.D., Organic Chemistry. Research Associate in Chemistry, Massachusetts Institute of Technology, Cambridge, Mass.

Burnett, Lee, M.S., Clinical Investigation, Biochemistry. Research Chemist, Sloan-Kettering Institute, New York, N. Y.

Bychowski, Gustav, M.D., Psychoanalysis, Psychosomatic Medical and Brain Pathology. Director, Department of Mental and Social Adjustment, Institute for Crippled and Disabled, New York, N. Y.

Cazzullo, Carlo Lorenzo, M.D., Biology, Neurology, Neurophysiology, Psychiatry. First Assistant, Neurologic Institute of Milan, Milan, Italy; Visiting Investigator, Rockefeller Institute for Medical Research, New York, N. Y.

Chaikoff, I. L., M.D., Ph.D., Physiology, Biochemistry. Professor of Physiology, University of California, Berkeley, Calif.

Chamberlin, Earl Martin, Ph D. Research Chemist, Merck & Co., Inc., Rahway, N. J.

Chernick, Sidney S., M.A., Physiology, Nutrition. Department of Physiology, University of California, Berkeley, Calif.

Cizek, Louis Joseph, M.D. Instructor, Department of Physiology, Coll. of Phys. & Surgs., Columbia University, New York, N. Y.

Clancy, Carl F., Ph.D., Virology, Bacteriology. Research Associate, Lederle Laboratories Division, American Cyanamid Co., Pearl River, N. Y.

Clark, Duncan W., M.D., Internal Medicine, Metabolism. Associate Dean, Long Island College of Medicine, Brooklyn, N. Y.

Cohen, Seymour Stanley, Ph.D. Assistant Professor of Physiological Chemistry, University of Pennsylvania, Philadelphia, Pa.

Contini, Renato, M.E., Anthropology, Psychology. Project Director, Research Division, New York University, New York, N. Y.

Converse, John Marquis, M.D. Surgeon in charge of Plastic Surgery, Department of Surgery, New York University Medical College, New York, N. Y.

Coriell, Lewis L., M.D., Ph.D., Virus Diseases, Bacteriology, Pediatrics. Instructor in Pediatrics, University of Pennsylvania Medical School, Philadelphia, Pa.

Crepea, Seymour Bernard, M.D. Research Fellow in Allergy, Roosevelt Hospital, New York, N. Y.

Daley, Mark J., M.D., Neurology. Yonkers, N. Y.

Daniel, Esther P., D.Sc., Cell Nutrition. Senior Biochemist, National Cancer Institute, Bethesda, Md.

Danowski, Thaddeus Stanley, M.D., Internal Medicine. Renziehausen Professor of Research Medicine, Pittsburgh School of Medicine, Pittsburgh, Pa.

Dattner, Bernhard, M.D., Neuropsychiatry. Assistant Clinical Professor of Neurology, Coll. of Phys. and Surgs., Columbia University; Associate Clinical Professor, New York University, New York, N. Y.

Deitch, Arline Douglas, M.A., Zoology, Embryology, Cytochemistry. Assistant, Department of Zoology, Columbia University, New York, N. Y.

Deitz, Victor R., Ph.D., Surface Chemistry, Adsorption and Adsorbents, Commercial Adsorbents. Physicist, Chemist, National Bureau of Standards, Washington, D. C.

Dorfman, Ralph I., Ph.D., Biochemistry, Endocrinology. Assistant Professor of Biochemistry, Western Reserve University School of Medicine, Cleveland, Ohio.

Dumm, Mary Elizabeth, Ph.D., Biochemistry. Instructor in Chemistry, New York University College of Medicine, New York, N. Y.

Dunnington, John Hughes, M.D. Professor of Ophthalmology, Coll. of Phys. & Surgs., Columbia University, New York, N. Y.

Durlacher, Stanley H., M.D. Chief, Section of Experimental Pathology, Medical Division, Army Chemical Center, Edgewood, Md.

Dy, Francisco J., M.D., Malariology, Parasitology, Consultant, Chief of the Malaria Section, U. S. Public Health Service, Rizal, Philippines.

Edelman, Isidore S., M.D., Medicine, Biology, Chemistry, Physics. Senior Resident, Neoplastic Division, Montefiore Hospital, New York, N. Y.

Edman, Pehr Victor, M.D. Docent in Biochemistry, Karolinska Institutet, Stockholm, Sweden.

Edson, John Newell, M.D., Internal Medicine. Instructor, Department of Medicine, L. I. Coll. of Medicine; Assistant Attending Physician, L. I. Coll. Hospital, Brooklyn, N. Y.

Edwards, George A., M.S., Comparative Physiology. Assistant Professor in Biology, Tufts College, Boston, Mass.

Eisen, Herman Nathaniel, M.D., Hypersensitivity, Immunochemistry. Resident in Medicine, 3rd (N. Y. U.) Medical Division, Bellevue Hospital, New York, N. Y.

Eisenberg, Leon, M.D., Medicine, Physiology, Psychiatry. Instructor in Physiology, University of Pennsylvania, Philadelphia, Pa.

Elek, Adalbert, Ph.D., Microchemistry. Director, Microanalytical Labs., Los Angeles, Calif.

Engel, Lewis L., Ph.D., Biological Chemistry, Steroid Hormones. Research Associate, Biological Chemistry and Medicine, Harvard Medical School, Boston, Mass.

Engelhardt, Edward L., Ph.D. Research Chemist, Sharp & Dohme, Inc., Glenolden, Pa.

Epstein, Morton B., Ph.D., Constitution of Petroleum, Physical and Thermodynamic Properties of Hydrocarbons. Research Associate, American Petroleum Institute, National Bureau of Standards, Washington, D. C.

Estren, Solomon, M.D. Research and Clinic Assistant in Hematology, Mt. Sinai Hospital, New York, N. Y.

Farber, Saul J., M.D., Kidney Physiology. Assistant Resident, 3rd Medical Division, Bellevue Hospital, New York, N. Y.

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Federn, Paul, M.D., Psychoanalysis, Psychiatry. New York Psychoanalytic Institute, New York, N. Y.

Feigin, William M., B.A. Teaching Fellow in Biology, Washington Square College, New York University, New York, N. Y.

Festinger, Leon, Ph.D., Social Psychology. Assistant Professor of Psychology, Massachusetts Institute of Technology, Cambridge, Mass.

Fischer, Ernst, M.D., Physiology. Professor of Physiology, Baruch Center of Physical Medicine, Medical College of Virginia, Richmond, Va.

Foldes, Eugene, M.D., Internal Medicine. Lecturer, N. Y. Polyclinic Medical School and Hospital, New York, N. Y.

Fontaine, Thomas Davis, Ph.D. Chemist, Biologically Active Compounds Division, Agricultural Research Center, Beltsville, Md.

Freeman, Benjamin, Ph.D. Chemist, Advance Chemical Products Co., Brooklyn, N. Y.

Freiman, Henry D., M.D., Medicine. Graduate Student, School of Medicine, University of Pennsylvania, Philadelphia, Pa.

Friedman, Lester David, M.D., Internal Medicine, Anthropology. Assistant Attending Physician, Montefiore Hospital, New York, N. Y.

Friedemann, Max W., M.D., Psychiatry, Psychology, Anthropology, Biology. Senior Psychiatrist, Pinewood Sanatorium, Crotona, N. Y.

Gey, George O., M.D., Cancer Research. Instructor in Surgery; Assistant Director, Division for Cell Physiology, Department of Surgery, Johns Hopkins Medical School, Baltimore, Md.

Glaubiger, Albert, M.S., Biochemistry. Chemist, Department of Health, New York, N. Y.

Glusman, Murray, M.D., Muscle Chemistry, Nutritional Deficiency. Assistant Resident in Neurology, Neurological Institute, New York, N. Y.

Goetzl, Franz R., M.D., Ph.D. Director, Department of Medical Research, Permanente Foundation, Oakland, Calif.

Goldberg, Samuel A., M.D., Ph.D., D.V.M., Pathology. Director of Laboratories, Presbyterian Hospital, Newark, N. J.

Golden, Abner, M.D., Cardiovascular Renal Research. 1st Asst. Resident in Medicine, Grady Memorial Hospital, Atlanta, Ga.

Goodhart, Robert Stanley, M.D., Biochemistry, Nutrition. Scientific Director, National Vitamin Foundation; Member, Food and Nutrition Board, National Research Council, New York, N. Y.

Goreau, Thomas, M.Sc., Biology, Physiology of Sea Animals. Research Assistant, Osborn Biological Laboratory, Yale University, New Haven, Conn.

Gould, Bernard, Ph.D., Enzymology, Biochemistry of Micro-organisms. Associate Professor of Biochemistry, Massachusetts Institute of Technology, Cambridge, Mass.

Green, Elizabeth Ufford, Ph.D., Cytology, Cancer. Biologist, Landeman Hospital Research Institute, Philadelphia, Pa.

Green, John D., B.Ch., Biologv. Blain Fellow, Wayne University Medical School, Detroit, Michigan.

Gregory, John Delafield, Ph.D., Organic Chemistry, Biochemistry. Research Associate, Rockefeller Institute for Medical Research, New York, N. Y.

Griffith, Wendell H., Ph.D. Professor of Biological Chemistry, St. Louis University School of Medicine, St. Louis, Mo.

Grolnick, Max, M.D., Allergy; Contact Allergy

(Skin). Associate-attending in Allergy, Jewish Hospital; Coney Island Hospital, Brooklyn, N. Y.

Guiteras, Albert F., Ph.D., Chemistry, Bacteriology. Research Co-ordinator, Foster D. Snell, Inc., New York, N. Y.

Gumbel, Emil J., Ph.D., Mathematical Statistics. Associate Professor, Brooklyn College, Brooklyn, N. Y.

Hampil, Bettylee, Sc D. Director of Virus Research, Medical Research Division, Sharp & Dohme, Inc, Glenolden, Pa.

Handley, Carroll A., Ph D. Associate Professor of Physiology and Pharmacology, Baylor University College of Medicine, Houston, Texas.

Harris, A. Sidney, Ph.D., Physiology, Heart and Nervous System. Associate Professor of Physiology, Baylor University College of Medicine, Houston, Texas.

Harris, Paul N., M.D. Pathologist, Eli Lilly & Co., Indianapolis, Ind.

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Heinemann, Heinz, Ph.D., Chemistry, Catalysis, Adsorption. Research Supervisor, Attapulgus Clay Co., Camden, N. J.

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Henle, Mary, Ph.D. Assistant Professor of Psychology, Graduate Faculty of Political and Social Science, New School for Social Research, New York, N. Y.

Hidnert, Peter, Ph.D , Physics. Chief, Thermal Expansion Section, National Bureau of Standards, Washington, D. C

Hinds, Ewart Gladstone, M.D , Ph.D., Psychiatry, Childhood Schizophrenia. Research Fellow, Psychiatry, New York University, New York, N. Y.

Householder, Alston S., Ph.D., Mathematical Biology, Psychology. Principal Physiscist, Clinton Labs., Oak Ridge, Tenn.

Huff, Jesse William, Ph.D., Metabolism of Nicotinic Acid. Research Biochemist, Sharp & Dohme, Inc, Glenolden, Pa.

Hummel, Katharine P., Ph.D., Histology, Embryology, Cancer Research. Research Associate, R. B. Jackson Lab., Bar Harbor, Maine.

James, Vera L., M S., Biology. Dept. of Pharmacology, Coll. of Phys. and Surgs., Columbia University, New York, N. Y.

Johnson, Julius E., Ph.D. Head, Chemotherapv Group, Biochemical Research Lab , Dow Chemical Co., Midland, Mich.

Jordan, William K., M.D., Cell Physiology. Research Assistant, Department of Biology, Princeton Universitv, Princeton, N. J.

Karon, Melvin L., M.S., Chemical and Biochemical Research. Research Chemist, Southern Regional Research Lab., New Orleans, La.

Kaufman, Jerome G., M.D. Assistant Professor of Medicine, N. Y. Medical College, New York, N. Y.

Kaufmann, Oliver W., M.S., Bacteriologoy, Antibiotics. Research Bacteriologist, Sheffield Farms, New York, N. Y.

Kaunitz, Paul Ehrlich, M.D., Biophysics. Fellow in Medicine, Mt. Sinai Hospital, New York, N. Y.

Kemp, Carol Louise, B.A., Biochemistry. Research Assistant, Thorndike Lab., Boston City Hospital, Boston, Mass.

Keston, Albert S., Ph.D., Biochemistry. Assistant Professor, New York University Medical School, New York, N. Y.

King, George J., B.A., Colloid Chemistry, Organic Surface Active Agents. Manager, Technical Service, Atlas Powder Co., New York, N. Y.

Klein, Samuel Harold, M D. Associate Attending Surgeon, Mt. Sinai Hospital, New York, N. Y.

Koepf, George F., M.D., Endocrinology. Associate in Physiology; Instructor in Medicine, University of Buffalo, Buffalo, N. Y.

Korey, Saul Roy, M.D., Medicine, Biology, Chemistry. Resident Neurologist, Medical Center, Neurological Institute of N. Y., New York, N. Y.

Kowal, Samuel J., M.D., Ph.D., Internal and Psychosomatic Medicine. Instructor, Boston University School of Medicine, Boston, Mass.

Krakauer, Joseph S., M.D., Medicine, Biology, Chemistry. Assistant Resident, Division of Neoplastic Diseases, Montefiore Hospital, New York, N. Y.

Kurtz, Russell Belden. Chemist, Hunter College, New York, N. Y.

Lanza, Anthony J., M.D., Industrial Medicine and Hygiene. Associate Medical Director, Metropolitan Life Insurance Co.; Professor of Industrial Medicine, N. Y. U. Medical School; Director, Institute for Industrial and Social Medicine, N. Y. U. Medical Center, New York, N. Y.

Larach, Simon, B.S., Chemico-physics of Luminescence in Solids. Research Chemist, R. C. A. Labs., Princeton, N. J.

Lawler, John A., Jr., M.D., Basic Sciences as Related to Medicine. Attending Surgeon, Bellevue Hospital; St. Vincent's Hospital; Assistant Professor of Clinical Surgery, New York University, New York, N. Y.

Lebish, Jacob, D.V.S., Biology, Anthropology. New York, N. Y.

Lehv, S. P., M.D., Neoplasms. Assistant Surgeon, Surgical Staff, Harlem Hospital, New York, N. Y.

Lemon, Julia G., M.S., Biochemistry. Research Associate, Lederle Laboratories Division, American Cyanamid Co., Pearl River, N. Y.

LeVeen, Harry Henry, M.D., Surgery, Physiology, Biochemistry. Instructor, Department of Surgery, New York University College of Medicine, New York, N. Y.

Lewis, Lena A., Ph.D., Medical Research, Adrenal Field, and Electrophoresis as Applied to Medical Problems. Staff Member, Research Division, Cleveland Clinic Foundation, Cleveland, Ohio.

Lewisohn, Richard, M.D., Cancer Research. Consulting Surgeon, Mt. Sinai Hospital, New York, N. Y.

Linder, Jerome M., M.D., Vasomotor Physiology. Clinical Surgeon, Kings County Hospital, Brooklyn, N. Y.

Lipetz, Leo E., B.E.E. Biophysics Research Assistant, University of California, Los Angeles, Calif.

Lipsett, Mortimer B., M.S., Biochemistry. Medical Student, University of Southern California, Los Angeles, Calif.

Littauer, Sebastian B., Sc.D. Professor of Mathematics and Chairman of Department, Newark College of Engineering, Newark, N. J.

Lobotsky, Julia, M S, Endocrinology. Research Assistant, Department of Physiological Chemistry, College of Medicine, Syracuse University, Syracuse, N. Y.

Locks, Matthew Owen, M.D., Internal Medicine. Westport, Conn.

London, Sol, M D. Staff Physician, Halloran V. A. Hospital, Staten Island, N. Y.

Low, Eva M., Ph.D., Organic Chemistry. Research Assistant, Department of Zoology, Yale University, New Haven, Conn.

Lowe, Adolph, Dr. Juris, Social Science. Director of Research, Institute of World Affairs; Professor of Economics, Graduate Faculty of Political and Social Science, New School for Social Research, New York, N. Y.

Lynd, Helen Merrell, Ph.D., Social Philosophy. Senior Member, Social Science Department, Sarah Lawrence College, Bronxville, N. Y.

MacCardle, Ross C., Ph.D. Senior Cytologist, National Cancer Institute, Bethesda, Md.

McCarty, Kenneth Scott, B.S. Cancer Research, Hyde Foundation, New York, N. Y.

McShan, William H., Ph.D., Endocrinology, Enzymology. Associate Professor of Zoochemistry, University of Wisconsin, Madison, Wis.

Maillard, Edgar R., M.D., Public Health, Medicine. Associate Pathologist, Charge of Branch Lab., N. Y. State Department of Health, New York, N. Y.

Mandel, Charles, M.D., Physiology, Biochemistry. New York, N. Y.

Maray, Hugh Z., M.D., General Surgery, Vascular Diseases. Instructor in Surgery, New York University College of Medicine, New York, N. Y.

Marks, Paul Alan, B.A. Medical Student, Coll. of Phys. and Surgs., Columbia University, New York, N. Y.

Marple, Charles Dalliba, M.D., Internal Medicine, Cardiovascular Diseases. Fellow, Vascular Disease Research, Cornell University Medical College; The New York Hospital, New York, N. Y.

Maver, Mary E., Ph.D., Biochemistry of Cancer, Proteolytic Enzymes. Senior Biochemist, National Cancer Institute, Bethesda, Md.

Maxwell, Morton Harrison, M.D., D.N.B., Medicine. Assistant Resident, Neurological Institute of N. Y., New York, N. Y.

Mayer, Gerda Gernsheim, Ph.D., Biochemistry. Research Associate in Chemistry, Mt. Sinai Hospital, New York, N. Y.

Mellquist, Enid H., B.A., Biochemistry. Technician, Rockefeller Institute for Medical Research, New York, N. Y.

Melnitsky, Ida, B.A., Biochemistry, Physiology. Teaching Fellowship in Biology, New York University, New York, N. Y.

Mendlowitz, Milton, M.D., Biology. Adjunct Physician, Mt. Sinai Hospital, New York, N. Y.

Messinger, William Josef, M.D. Associate in Research, New York University School of Medicine, New York, N. Y.

Michel, John, Aviation, Rocketry, Electronics, Archaeology. Associate Editor, Air Trails, Flying Aces; Editor-in-Chief, Flying Age Traveler, New York, N. Y.

Miller, James A., Ph.D., Biochemical Aspects of Cancer. Assistant Professor of Cancer Research, McArdle Memorial Lab., University of Wisconsin, Madison, Wis.

Minor, Allen H., M.D., Cancer Research, Metabolic Aspects. Trainee, National Cancer Institute, Memorial Hospital, New York, N. Y.

Mirone, Leonora, M.S., Nutrition. Student, Department of Biochemistry, Fordham University, Bronx, N. Y.

Morgareidge, Kenneth, Ph.D, Biochemistry, Nutrition, Vitamins. Director, Research and Control Labs., Nopco Chemical Co., Harrison, N. J.

Moses, Montrose J., M.A., Zoology (Cytology). Asst., Department of Zoology, Columbia University, New York, N. Y.

Munro, Muriel Platt, Ph.D., Blood Coagulation, Protein Chemistry. Research Chemist, Charlotte Drake Cardeza Foundation; Dept. of Medicine, Jefferson Medical Hospital, Philadelphia, Pa.

Murphy, Wallace B., M.D., D.Sc., Surgery, Peripheral Vascular Disease. Assistant Professor of Clinical Surgery, New York University; Associate Visiting Surgeon, Bellevue Hospital, New York, N. Y.

Myers, Gordon B., M.D. Professor of Medicine, Wayne University College of Medicine, Detroit, Mich.

Nelson, Carl Truman, M.D., Bacteriology, Immunology, Clinical Medicine. Instructor in Dermatology, Columbia University, New York, N. Y.

Nicholas, John Spangler, Ph.D., Embryology. Sterling Professor of Biology; Chairman, Department of Zoology, Osborn Zoological Lab., Yale University, New Haven, Conn.

Nielsen, Edward, Ph.D., Biochemistry. Biochemist, Lederle Laboratories Division, American Cyanamid Co., Pearl River, N. Y.

Nigg, Clara, Ph.D., Bacteriology, Virology. Head, Virus Labs., E. R. Squibb & Sons, New Brunswick, N. J.

Nixon, Norman, M.D., Psychiatry. Member, Attending Staff; Council, Child Development Center, New York, N. Y.

O'Kane, Daniel J., Jr., M.S., Bacteriology. Fellow, Lab. of Bacteriology, Cornell University, Ithaca, N. Y.

Oroshnik, William, Ph.D., Organic Syntheses, Medicinal Chemistry. Director, Division of

Organic Chemistry, Ortho Research Foundation, Raritan, N. J.

Owen, Philip S., M.D., Medical Research; Growth, Neoplasia. Executive Secretary, Committee on Growth, National Research Council, Washington, D. C.

Page, Irvine H., M.D., Cardiovascular-renal Disease. Director of Research, Cleveland Clinic, Cleveland, Ohio.

Parker, Julius G., M.D., Internal Medicine. Assistant Resident in Pathology, Montefiore Hospital, New York, N. Y.

Parker, Robert F., M.D., Medicine, Microbiology. Associate Professor of Medicine, Western Reserve University, Cleveland, Ohio.

Perera, George A., M.D., Med.Sc.D. Assistant Professor of Medicine, Coll. of Phys. and Surgs., Columbia University; Assistant Attending Physician, Presbyterian Hospital; Vanderbilt Clinic, New York, N. Y.

Perr, Herbert M., M.D., Internal Medicine. Assistant Resident in Chest Diseases, Montefiore Hospital, New York, N. Y.

Peyser, Pincus, B.A., Biochemistry. Research Assistant, Division of Experimental Medicine, University of Vermont School of Medicine, Burlington, Vt.

Pines, Kermit L., M.D., Medicine, Hypertension, Nicotinamide Metabolism. Student, Department of Medicine, Coll of Phys. and Surgs., Columbia University, New York, N. Y.

Pollack, Abou D., M.D., Medicine, Biology. Pathologist, Camp Detrick, Frederick, Md.

Popino, Robert P., B.S., Antibiotics, Chemotherapeutics. Supervising Chemist, Heyden Chemical Corp., New York, N. Y.

Potor, Aurelio, M.D., Internal Medicine, Clinical Investigation. Fellow in Medicine, Sloan-Kettering Institute, New York, N. Y.

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SECTION OF GEOLOGY AND MINERALOGY

SEA LEVEL

By HOWARD S. RAPPLEYE*

Going back, in our imagination, to the early stages of civilization on this earth, we may find a man having established his home somewhere in the flat country just back of a gently-sloping sand beach. He must have done this for reasons which seemed good to him, and if we pursue our inquiry further, we would suggest that they may have been about as follows:

(1) The flat fertile country of the lowlands would make it easier for him to grow the food he needed than if he cultivated the steeply-sloping hillsides farther inland.

(2) He would be near an additional source of food supply, to be taken from the sea.

(3) He would have more ready means of communication with his like-minded fellows by means of water transportation.

Our remote ancestor, then, has established his home on the coast and it is not long before he has learned from experience that if he lands his dugout on the beach when the water's edge is far out from his door and does not pull his canoe well up on the beach, it will probably float away within a matter of relatively few hours. Conversely, if he lands when the water's edge is in close to his door he will, if he wishes to set sail again when the water has receded, have to drag his boat a long way down the beach before it can be floated.

This state of affairs is *forced* on his attention and he eventually notices that there is a somewhat rhythmic quality to the behavior of the water's edge with relation to the tree line back of the beach.

He talks the matter over with his family and perhaps with some of his neighbors. They agree that there is a tendency toward a repetitive cycle in the events concerning the behavior of the water's edge. Someone, perhaps one of his great-grandchildren, eventually tumbles to the fact that this cycle of events comes fairly close to repeating itself every time the moon goes through its changes from new moon to half moon, full moon, half moon and back to new moon again. Once this idea has dawned on these people and they seek to prove that it is a fact, the study of the tides has begun.

As it is not within the scope of this discussion to trace the history of tidal in-

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* U. S. Coast and Geodetic Survey, Washington, D. C. This paper, illustrated by lantern slides, was presented at the meeting of the Section on November 3, 1947.

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vestigations from the beginning up to the present time, the best we can do is to start out with the knowledge accumulated through the ages and to try and see of what use this knowledge may be to us and what practical results may be obtained from all the investigations that have been completed, as well as from those that are still going on. We may even try to predict what may happen in the future to that much talked-about but too little understood surface which we glibly call *sea level.*

Those who dwell along the shores of the oceans and tidal estuaries will readily admit a keen interest in the tides and there need be no argument about it. That Mrs. John Doe, of Peoria, Illinois, has any direct interest in matters pertaining to the tides is less obvious, though a little reflection may serve to prove that this is so. For example: the accuracy and quality of nautical charts is largely dependent upon an accurate knowledge of the tides and their behavior; good nautical charts, properly used, tend to reduce greatly the dangers to shipping; increased safety for shipping tends to reduce marine insurance rates; and these reduced insurance rates decrease the ultimate cost to the consumer of goods brought from foreign ports. I believe that it will now be agreed that Mrs. Doe pays less for her Italian-lace table cloth or her Chinese-silk kimono than she would have had to pay if a lot of observation and analysis had not gone into the study of the tides.

Since it is obviously much easier to understand a Chinese if you know what he is talking about, it may be well, at this point, to define sea level and explain briefly how it is determined.

Mean sea level is defined, by those best qualified to know just what it really is, as "the average height of the surface of the sea, all stages of the tide considered." This definition implies measurement of the height of the water surface with reference to some point or index. Since mean sea level is determined by measurement, the resulting data must perforce apply to the surface of the sea at the place where the observations were made. Further, the length of the series of observations has a great effect on the accuracy of the resulting numerical value. For these reasons, it is quite customary, in defining mean sea level, to state the length of the series of observations on which the numerical value is based. Thus, we might say that mean sea level at Fort Hamilton, N. Y., based on continuous observations for 38 years, from 1893 to 1930, is 8.98 feet below bench mark 20.

The modern method of determining mean sea level is to establish a tide gauge and operate it for a considerable period of time. This gauge is simply an installation which produces a continuous graphic record of the rise and fall of the surface of the water, with an appropriate datum line running the length of the continuous curve and suitable time marks spaced at intervals of one hour along the curve.

FIGURE 1 shows a schematic diagram of the installation and you will note that the recording instrument is mounted on the table or bench inside the tide-gauge house, and that the bench and instrument are placed over the float well so that a vertical wire leading from the float to the recording instrument will hang in the center of the well and keep the float from dragging on the sides of the well. Two counter-weights show in the diagram. One is in a bight in the wire leading from the float over a spool on the gauge and through suitable pulleys to the counterweight and then to an anchorage in the roof. The other furnishes the power for the take-up spool to keep the paper wound up properly and to relieve some strain on the clock that controls the feeding of the paper over the main roller, along an element of which the recording pencil moves back and forth in trac-

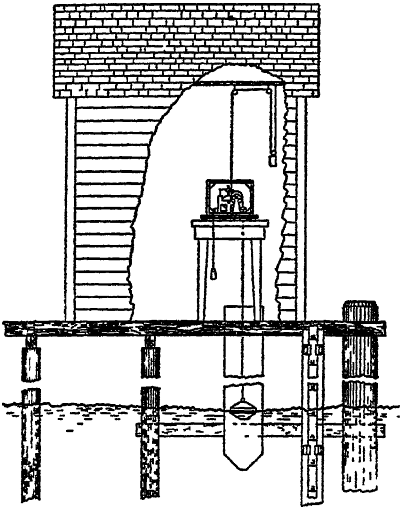

FIGURE 1.

ing the curve of the rise and fall of the float in the well.

In a close-up of the recording gauge, we would notice two clocks, one causing the paper to feed over the main roller at the rate of one inch per hour and the other producing the time marks at hourly intervals on the curve traced by the recording pencil. A fixed pencil traces a datum line the length of the curve as the paper passes under it.

The curve is traced by means of a pencil which is made to move along an element of the main roller by means of a spirally-grooved shaft which in turn is directly connected to the spool over which the wire passes in leading from the float to the counter-weight. This counterweight keeps the wire taut at all times and prevents slippage on the spool, which is grooved and carries several turns of wire at one time to insure sufficient friction between the wire and the spool.

The tide staff is simply a long board, graduated in feet and tenths from the bottom upward, and is mounted in a vertical position on some suitable structure so that its zero is well below the lowest expectable low stage of the tide at that station. This staff is read daily at the time the gauge is inspected and serviced, and the staff reading is noted directly on the curve at the point indicated by a manually-made jog, similar to an hourly time mark, put in the curve at the time the staff reading was taken. These staff readings serve to define the height of the datum line on the curve with respect to the zero of the staff gauge.

At suitable intervals of time, the staff is connected, by means of spirit leveling, with the bench marks on shore. These are set at suitable locations on shore and near the tide station. They serve to furnish the permanent points to which the observations are referred.

Should the staff be displaced by a storm or some other accident, a new one can be installed and the observations kept continuous by leveling to the new staff from the bench marks and determining accurately the height difference between the zero of the original staff and the zero of the new one. Properly handled, the series of observations at a station may be kept on a consistent basis, even though the staff may have been dislodged once or even many times during a long series of observations.

At many places, the tide is a semidaily phenomenon, the surface of the water going up and down again twice in one day. We may look at the tide curve for New York, say, for the month of June, 1934. The actual curve traced by the recording gauge is 24 inches long for each day and cannot be more than about 12 inches high, even when the full available height limits are utilized. Suitable reduction mechanisms will reduce even a 30-foot tide to curve height of a foot or less. Thus, it will be seen that the curve in question has been greatly compressed in the matter of time. Our illustrations are usually made up in this way to conserve valuable space.

While the semi-daily tide is probably best known, especially to the layman, this comes about, not because it is the most prevalent type of tide but because it happens to be the type of tide which occurs at most places on the shores of the Atlantic Ocean and has been observed at more stations. The mixed tide is certainly more prevalent along the shores of the Pacific Ocean than the semi-daily tide and, therefore, is probably the most widespread type of tide when we consider the whole aggregation of all the shore lines of the world. The true daily tide is, without doubt, less frequently encountered than either the semi-daily or mixed tides.

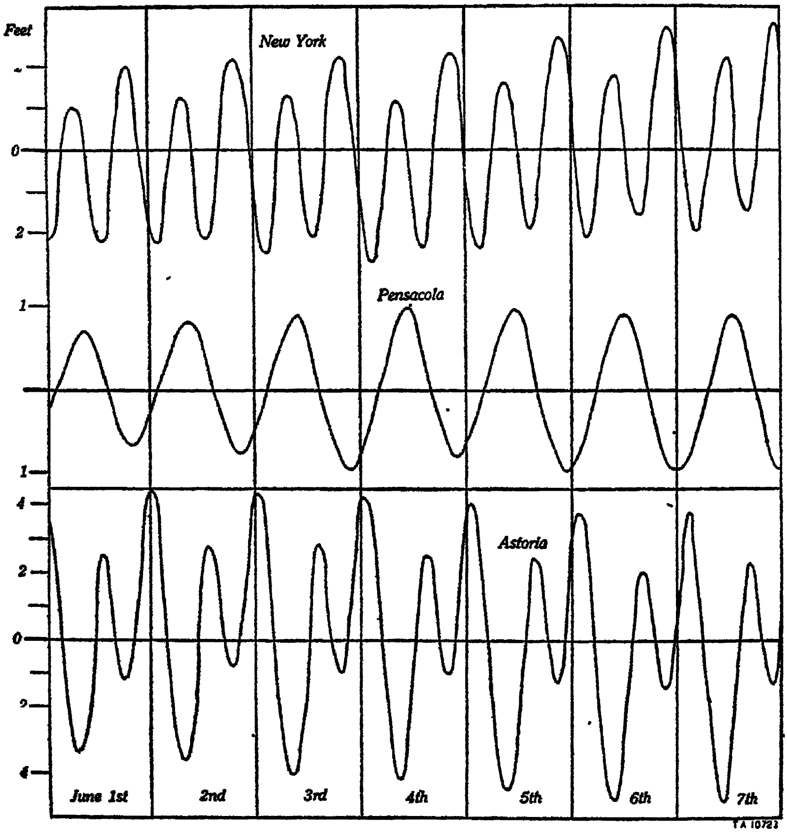

FIGURE 2.

If we look at the tide curve for Seattle, Washington, also for the month of June, 1934, we can see that the tide at Seattle is quite different from that in New York and, also, that the character of the tide changes quite materially during the month.

FIGURE 2 shows a week of record for each of three stations, New York, Pensacola, and Astoria. These, in order from top to bottom, represent typical examples of the semi-daily, daily, and mixed tides for purposes of comparison.

It will be noticed that, in the schematic diagram of the tide-gauge installation (FIGURE 1), the bottom of the float well is tapered to a point. This is to indicate that the bottom of the well is constricted to a small opening to damp out wave action of short period and to produce a smooth curve free from the violent short-period oscillations that would be produced by severe storms. This not only serves to smooth the curve and permit more accuracy in scaling the hourly heights from the curve, but saves wear and tear on the gauge itself. If the bottom of the float well were wide open, wave action might cause the float to rise so rapidly that the counter-weight could not keep sufficient tension on the wire, and the wire would come off the grooved wheel at the end of the spirally-grooved shaft. The result would be that the gauge would be out of action until again serviced and there would be a gap in the record. Further, the small opening in the bottom of the float well is *actually* at the lower extremity of the conical bottom to permit finding the opening quickly when it is necessary to pass a cleaning rod through it to dispose of barnacles or other matter that may have partially clogged the opening.

By means of the recorded staff readings, the height of the datum line on the curve is determined and the hourly heights are scaled and tabulated. These are averaged to produce the final result. It may be interesting to note that the 38 years of observations at Fort Hamilton, to which we previously referred, involved at least 456 marigrams, each at least 60 feet long, and the scaling and averaging of over 330,000 hourly heights.

The averaging of the 24 hourly heights for a single day results in a daily mean sea level. For the rather obvious reasons of wind, variation of barometric pressure, etc., this will vary considerably from day to day. In a study of actual records of the values of daily mean sea level at various locations, it will be noticed that the daily mean sea levels vary considerably from day to day, in places as much as a foot or more between two consecutive days. Obviously, if we want an *accurate* determination of mean sea level we will have to extend our observations over a longer period of time.

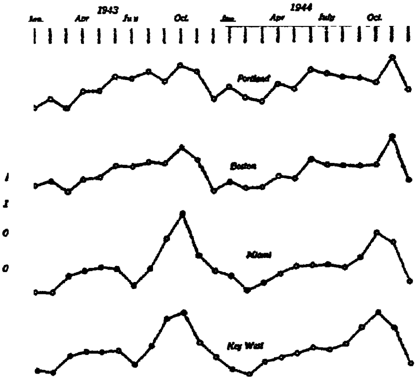

FIGURE 3.

The averaging of the scaled hourly heights for a month results in a monthly mean sea level. Certain typical observations of monthly mean sea levels are show in FIGURE 3. Here, we have shown monthly mean sea levels for a two-year period for each of four Atlantic Coast stations: Portland, Maine; Boston, Massachusetts; and Miami and Key West, Florida. A close examination of FIGURE 3 will show two things:

(1) There is still a considerable variation from month to month, in some places approximately half a foot.

(2) There is obviously a seasonal effect.

That this seasonal effect is not merely an apparent condition but actually exists, is shown by FIGURE 4. Here, we have *daily* mean sea levels for the months of January and July, 1939, at Atlantic City, N. J., and it is rather obvious that during the month of January the behavior of the

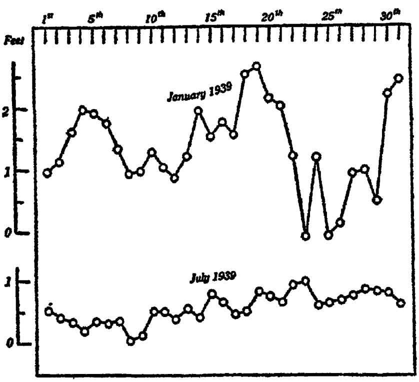

Figure 4.

tide at that place was much more erratic than during the month of July.

It now looks as if a two-year series of observations were not enough to result in a really accurate determination of mean sea level at a given place. In an analysis of annual mean sea levels for various stations on the Atlantic Coast, we find that the variation from year to year is much less than the variation from day to day or month to month, but that even there considerable variation (up to as much as 0.2 ft.) occurs between successive annual means at these stations.

Theoretically, a series of sea levels taken over some twenty years should produce a resulting mean sea level which is accurate to within a hundredth of a foot, and, actually, in most localities, this is true. However, such a series, when examined closely, will show an apparent upward trend with the passage of time at various stations on all coasts of the United States, so that it looks as if it were still necessary to keep comparison stations going continually at many points along the coasts in order to study the long-term behavior of this reference surface, which is apparently continually developing some new whim or twist. But more of this later.

It will be evident from the foregoing statements that the task of making a really accurate determination of mean sea level at a given station is a long and tedious process. It requires the establishment and maintenance of an automatic water-level recording instrument, daily inspection, recording the daily reading on the staff and the time at which the reading was taken, the periodic cleaning of the float well, occasional replacement of the staff or even of the recording instrument, and periodic levelings to the staff from the bench marks on shore. In the office, there are thousands of hourly heights to be scaled from the marigrams each year, monthly means to compute and record, annual means to be taken, and all this costs money. The cost of a really accurate determination of mean sea level is such that it is readily understood why there are not more long-period tide stations in operation on the coasts of this and other countries.

Mean sea level having been determined at a station, we are now at liberty to consider some of the associated datum planes or surfaces that are intimately connected with the principal plane.

Half-tide level is, as might be expected, the average of all the high and low points on the tide curve. This plane or surface of reference is usually the one that is computed and published in lieu of mean sea level at a station where only a short series of observations is available. It is a fairly close approximation to mean sea level, but may vary from the value of mean sea level, which might be computed from the same series of observations, by as much as 0.2 ft. or more in some cases.

The navigator is interested in knowing the least depth of water over shoals and bars. For this reason, the datum to which soundings are referred in the preparation of nautical charts is usually a low-water datum. On the Atlantic Coast, and in other places where the daily or semi-daily tides are the rule, the plane of reference to which soundings on charts are reduced is mean low water and, mathematically, this is the average of all the low waters during the period of the observations.

In localities where the mixed tide occurs, the plane of reference for soundings is usually mean lower low water, since there is usually considerable difference between the heights of the two low waters which occur in a single tidal day at such stations.

The engineer who is interested in the drainage of wet lands near the coast or in disposing of storm water in a coast city is interested not in a low-water datum but in a high-water datum, since the effectiveness of his drainage or disposal system is decreased by high water in the bay or stream into which he is running his unwanted water. Accordingly, the plane of mean high water is used in the vicinity of stations having semi-daily or daily tides and the plane of mean higher high water is the one in which interest is centered in case the mixed tide is encountered in connection with a drainage problem.

Further, the extreme tides are of interest in connection with navigation when unusual conditions are encountered and in drainage when periods of extreme high water are apt to have a serious effect on the drainage system or even cause a backing up of unwanted water to make a flood. Accordingly, the extreme high and low tides are usually noted and published along with the rest of the information concerning a particular tide station.

Figure 5 is of interest in that it shows what can happen to a tide curve at a given station when a really unusual set of conditions exists for a more or less short period of time. This curve shows the fluctuations of the water surface at Honolulu for the four days, February 2-5, 1923, and reflects the effect of the earthquake which occurred on February 3, 1923, off the coast of Kamchatka, about 2500 miles from Honolulu. The first sea wave arrived at Honolulu about six hours after the earth quake, and the subsequent fluctuations of the water surface were of considerably greater magnitude in a matter of minutes than the normal mean range of the tide at that place.

Lastly, in connection with the published data, the descriptions of the bench

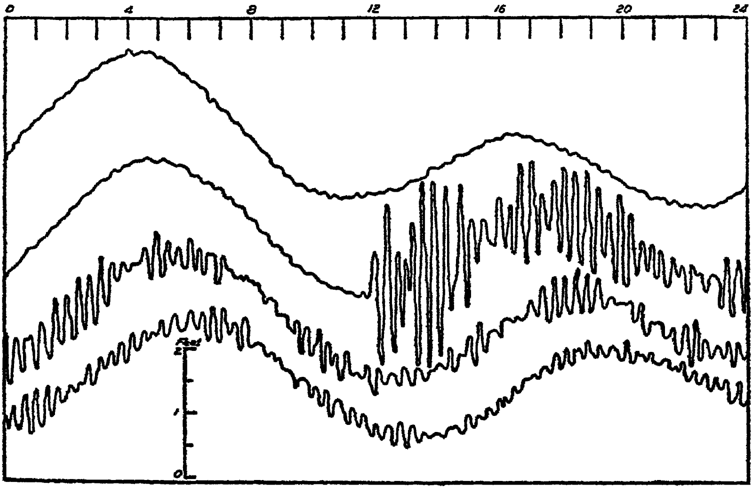

Figure 5.

marks to which the tide staff is referenced are listed with their elevations given above one of the reference planes at the station, usually one of the low-water planes. The elevations can be transferred from one datum plane to another by simple arithmetic, as the differences between the various planes may be obtained by simple subtraction.

In all of the preceding discussion, we have tacitly assumed that everything is working smoothly, that the shore line is stable, the plane of mean sea level once accurately determined remains fixed, and we have not made much mention of the effects of wind, currents, variations in barometric pressure, fresh water effect in tidal streams, channel effects when stations are located in bays or harbors having narrow entrances, stability of the land, or variations in the mean level of the sea itself.

Obviously, wind action is most noticeable in form of waves breaking on the shore. We have already discussed the matter of avoiding this difficulty by constricting the opening in the bottom of the float well. Another problem arises in connection with the known fact that a constant onshore wind will tend to pile water up on the beach and an offshore wind, if long continued, will cause a distinct lowering of the surface of the water. Even under less than extreme conditions this is especially noticeable in bays with narrow entrances and has its effect on the resulting values computed for the various reference planes, especially at stations where only a relatively short series of observations has been made.

The direction of prevailing wind in the United States is, in general, from west to east, which may account partially for the observed "tilt" of mean sea level, to be discussed later.

Naturally, a tide station located considerably inland from the mouth of a tidal stream is bound to be under the influence of the fresh water coming down the stream. Floods may raise the water level by many feet for short periods of time and, as these extreme high waters are all included in computing the final values of the reference planes, they have their effect and tend to make the reference planes, at stations not on the open coast, stand higher than the corresponding planes at the open-coast stations. Also, fresh water is of different density than salt water. This causes a slight added complication when dealing with a station at which a large amount of fresh water passes the gauge.

At this point, it would be reasonable to ask why, if so much trouble is encountered with tide stations that are not on the open coast, not all stations are built outside, where they will not encounter all of the difficulties?

There are two principal answers to that question:

(1) A tide station on the open coast is very costly to construct and difficult to maintain, except perhaps where one is lucky enough to find a place like Atlantic City where piers actually extend out into the open ocean, for the station usually requires that a special structure be built to support it, and it is very apt to be put out of commission by storms which require the rebuilding of the station and result in a loss of record. Furthermore, in order to reduce soundings in bays and tidal streams, we need the local reference planes and not planes that might be determined on the open coasts and projected into the bays and up the tidal streams. For example, the mean water level at Washington, D. C., is approximately 0.4 foot higher than the mean sea level at Hampton Roads, Virginia.

(2) Also, the *times* of high and low waters are quite different at different stations and, in reducing soundings to the adopted reference plane, simultaneous tide-gauge readings are needed at a

location close by the area which is being sounded by the hydrographic party.

We have touched upon the needs of the navigator and discussed the reduction of soundings to the adopted reference plane. This provides the mariner with a knowledge of what is apt to be least water over the bars and shoals which he will encounter. However, especially at harbor entrances, a deep-draft vessel may be required to wait for high tide to successfully cross the bar. This requires that the navigator be provided with the times and heights of high and low waters at many places in advance of his sailing. The tide tables published by the Coast and Geodetic Survey, and by other similar organizations in other countries, give the mariner this information based on predictions of the time and height of high and low water at many stations.

The observations which we have been discussing are also subjected to a mathematical process known as harmonic analysis, and certain constants are determined for each long-period station. From these constants can be worked out the predicted tides for future use. This, in the old days, was a really laborious process, but by the use of the tide-predicting machine which was developed in the offices and shops of our Bureau the whole process becomes a mechanical one.

The necessary constants for a station can be set in the machine and checked and the predictions for a year for that station completed in one day. Usually, the predictions are made two or three years in advance so that the information may be printed and distributed in advance of the time it will be needed by the mariner. It is rather remarkable what accuracy is obtainable in these predicted values. Unless storm conditions are encountered, the predicted values are usually checked, when the tide observations are actually made at the station, within a few minutes in the *times* of high and low waters and within a tenth of a foot or so in the *heights* of high and low waters.

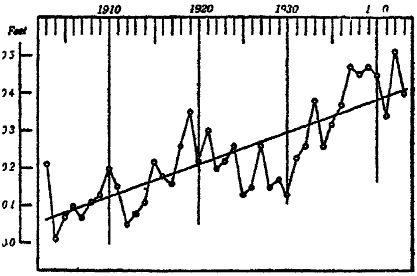

FIGURE 6.

For the stations on the Atlantic Coast, there was an evident upward trend of the mean sea level surface on the staff at each station. FIGURE 6 shows the annual mean sea levels at Baltimore, Maryland, from 1903 to 1943, and there has been drawn through the irregular curve representing these annual mean sea levels a straight line which was located on the other curve by the method of least squares. This, *if* we assume that sea level has been rising at a uniform rate during the forty-year period, gives the rate of rise, which appears to be something of the order of size of 0.01 ft. per year.

FIGURE 7 shows the result of treating the same basic data by the method of the moving mean. With this treatment, it appears that the rise has not been uniform, as was *assumed* in the preparation of FIGURE 6, but slow during the period prior to about 1928 and much more rapid from then on to the end of the series in 1943. The greatest slope of the curve shows that, during the last fifteen years,

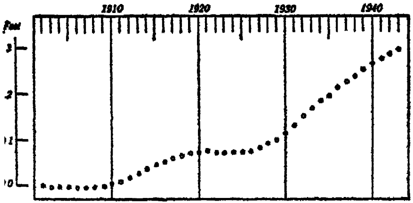

FIGURE 7.

the rate of rise has been of the order of size of 0.015 ft. per year. The same state of affairs is known to exist at all of the long-series stations on the Atlantic and Pacific Coasts of the United States.

One may well raise the question as to whether or not this represents a rising of sea level or a settlement of the land. It could be either, or even a combination of both. However, it hardly seems possible that so large a land mass as the continental United States could be sinking so uniformly as to produce the observed changes in the relationship between the tide staff and mean sea level at so many different tide stations so widely scattered along the coasts of this country. Furthermore, it is a known fact that the glaciers of the world have been receding for a considerable period of time. The melting back of the glaciers and a shrinking of the boundaries of the ice caps at the poles would certainly have some effect on the elevation of mean sea level throughout the world. Also, the soil conservationists have been crying for some time for greater efforts in holding moisture in the soil. Water tables have lowered seriously in many places. All of this is in the right direction to account, partially at least, for the apparent rise in sea level.

The geologist has a habit of measuring time in units of hundreds of thousands or perhaps even millions of years, and finds evidences of very great changes in the level of the sea, the land, or both. As geodesists and tidal specialists, we have to base our conclusions, at present at least, on data which, except in very few instances, barely cover a period which could be called half a century. This is a mighty short base from which to expand. Any small observational errors are greatly magnified when we attempt to do this, while much larger errors become relatively insignificant when contracted from a time base of even 100,000 years.

The fundamental vertical control net of the United States is a very tight network of lines of first-order leveling throughout the country. These have been run in such a way that they form closed loops of spirit leveling and a considerable number of spirit-level connections between the tide bench marks on which our connections to mean sea level are made. An idea of the accuracy of the leveling may be given by stating the fact that, when the combined level nets of the United States and Canada were adjusted simultaneously in 1929, the *average* rate of correction which it was necessary to apply to the approximately 65,000 miles of leveling included in the adjustment was of the order of size of less than 0.2 mm. per kilometer.

It has long been known that geodetic leveling between tide stations did not always produce checks that were within the expected limits of accuracy of the leveling and/or the accuracy of the tide observations. For example, accurate spirit leveling across the Isthmus of Panama shows that mean sea level on the Pacific side is about 3/4 ft. higher than mean sea level on the Atlantic side. The same state of affairs has been observed between the north and south coasts of France, the mean level of the sea on the north coast standing higher than that on the south coast by an amount comparable with the findings in other parts of the world. The same phenomenon was also observed on the coasts of England and Wales, where very accurate spirit leveling failed to agree with the tide observations by amounts considerably greater than could be ascribed to errors in the leveling, errors in the tide observations, or both combined.

In 1927, a special adjustment of the leveling net of the United States was made to test this condition for the coasts of the United States. The problem was attacked by adjusting all of the land circuits of leveling to consistency without holding *any* sea-level connections. Then, with an observed difference from Galveston to the nearest junction in the net, and observed differences from other junctions nearest to the tide stations down to the tidal connections, we were able to arrive at spirit-level elevations for the observed planes of mean sea level at the various tide stations, all based on the observed mean sea level at Galveston. The results showed that, apparently, the sea-level surface, as defined by the observed planes of mean sea level at the various tide stations, had a marked tendency to stand higher to the northward along both the Atlantic and Pacific coasts and higher to the westward along the Gulf Coast. As might be expected from the upward trend westward along the Gulf Coast, the average for the Pacific was considerably higher than that for the Atlantic Coast.

In 1929, a further study was made by making a similar investigation of the combined level nets of the United States and Canada. The results further confirmed the findings of the 1927 Special Adjustment and are shown in TABLE 1, in which the 0.00 difference has been shifted to St. Augustine, simply to rid the table of negative values. The differences between the stations remain the same.

The upward trend along the east and west coasts is quite evident and extends on north along the coasts of the Dominion of Canada. The upward trend along the Gulf Coast to the westward is also evident and the fact that the average height of mean sea level on the Pacific side is considerably higher than the average for the Atlantic stations is also quite plainly shown. As to *why* this condition exists, there are several factors which may be considered. The general direction of prevailing winds may partially account for it. Differences in salinity, variations in average barometric pressure, configuration of the coast lines and, perhaps, ocean currents may each contribute their share to the sum total of this variation of the mean-sea-level surface from its theoretical form. Errors of observation in both the leveling and in the tidal observations are present, of course, but these alone are hardly sufficient to account for the observed differences.

TABLE I

RELATIVE ELEVATIONS OF MEAN SEA LEVEL DETERMINED BY THE 1929 SPECIAL ADJUSTMENT

Station	*Meters*
Galveston	0.27
Biloxi	0.18
Pensacola	0.22
Cedar Keys	0.19
St. Augustine	0.00
Fernandina	0.07
Brunswick	0.11
Norfolk	0.13
Old Point Comfort	0.01
Cape May	0.29
Atlantic City	0.30
Annapolis	0.10
Baltimore	0.09
Perth Amboy	0.28
Fort Hamilton	0.30
Boston	0.34
Portland	0.39
Yarmouth	0.32
Halifax	0.35
Father Point	0.47
San Diego	0.60
San Pedro	0.58
San Francisco	0.61
Fort Stevens	0.86
Anacortes	0.72
Seattle	0.75
Vancouver	0.77
Prince Rupert	0.85

Such is the present state of affairs, as far as it is possible to picture it in this brief space. We have a host of unsolved problems yet to be worked on. Nature has been keeping at least a crude record of what has been happening for hundreds of thousands of years, for geologists to read. Obviously, the tidal and geodetic observations, which have been carried on intensively in this country for perhaps not more than a half century, give a more detailed picture and permit more accurate quantitative deductions than nature's geologic record. However, the size of the quantities we are trying to measure are dangerously close to the limits of error of the observations themselves, when the short time base on which we now have to work is considered. Given a chance to make accurate tidal and geodetic observations for even *one* thousand years, we shall be able to show results of considerable significance. In the meantime, the geologist will have to make the best of what little we can offer to prove or disprove the interpretations which he makes of nature's record, written in the geolocial structure of the earth.

SECTION OF BIOLOGY

RELATIONSHIPS OF NONHEMOLYTIC AND VIRIDANS STREPTOCOCCI IN MAN

By GEORGE H. CHAPMAN*

In cultures made by the average bacteriologist, a minute colony on a blood agar plate is taken as a streptococcus and any discoloration of the surrounding medium is supposed to differentiate it into "species," although slight differences in ingredients of the culture medium have a profound effect on the blood-altering and other properties of streptococci. Consequently, few reports on the presence of this organism have any significance. I shall try to show how it is possible not only to isolate streptococci more frequently and more surely, but to determine the significance of α- and γ-hemolytic types in man with a high degree of precision. Work with β-hemolytic types presents an entirely different problem.

This report discusses the results of 23 years of intensive study and omits experimental data, much of which has been published elsewhere.[1-30]

Methods. If an apparently pure culture of streptococci is plated so as to give discrete colonies and they are tested separately for different properties, they will be found to constitute a kaleidoscopic mixture. Some may ferment a particular carbohydrate, some may be toxigenic, etc. Moreover, if a toxigenic colony is replated, the daughter colonies, likewise, may vary in toxigenicity. Toxigenic streptococci are particularly significant in man because they are associated with tissue damage and because their cultures make decidedly better antigens than do those of non-toxigenic streptococci. Therefore, precise measurement of the toxigenicity of streptococcal cultures assumes considerable significance. To accomplish this successfully, any degeneration of toxigenicity must be reduced to a minimum. A considerable reduction can be achieved by using brain heart infusion (Difco) for subculture, inoculating it with a single colony to secure a pure undissociated culture rapidly, harvesting the culture in the late logarithmic phase, and transferring it immediately to the refrigerator and testing it promptly. Under these conditions, the proportion of toxigenic colonies in a culture is an accurate measure of its degree of toxigenicity. In general, this proportion of toxigenic colonies in a culture from the body is directly related to the *severity* of intoxication in the tissue from which the culture was obtained. The total number of streptococci in that part is an indicator of the *amount* of toxic material it contains. Such toxigenic organisms probably grow better in the body than do non-toxigenic ones, because usually the higher the total number of streptococci in a particular tissue the greater the proportion of toxigenic colonies.

In acute infections or acute exaccerbations of chronic infections, the organisms become more "virulent" (invasive), but there is little change in toxigenicity.

These toxins can damage or sensitize any tissue in the body. The more intense the toxigenic power of the organisms, the greater the tissue damage. When the damage is severe enough to affect local resistance materially, non-pathogenic bacteria can then invade the tissues. Hence, coliforms and other non-pathogenic bacteria are found in habitats foreign to them usually when the streptococci in that part are highly toxic.[5]

Accurate determination of the total number and proportion of toxigenic colonies in a sample depends on (1) satisfactory quantitative isolation methods and (2) satisfactory methods for testing the toxigenicity of the organisms isolated.

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* Clinical Research Laboratory, New York, N. Y. Aided by grants from the Ophthalmogical Foundation, Inc. This paper, illustrated by lantern slides, was presented at the meeting of the Section on November 10, 1947.

The isolation method used is as follows: Spread 0.01 ml. of appropriate undiluted or serial decimal dilutions of the sample on the surface of a plate of tellurite streptococcus medium, which has the following composition: Proteose peptone No. 3, 5 grams; proteose peptone, Difco, 5 grams; Bacto-tryptose, 10 grams; Bacto-agar, 15 grams; saccharose, 50 grams; K_2HPO_4, anhydrous, reagent grade, 1 gram; trypan blue, 1.0 per cent, 7.5 ml.; crystal violet (Commission Certified), 0.10 per cent based on dye content, 0.8 ml.; water, 1,000 ml. Sterilize as usual. When cool enough to pour, add 0.20 ml. of 5.0 per cent *unheated* "certified" potassium tellurite and pour into 40 plates. Since the amount of tellurite is critical, I have prevailed upon the Difco Laboratories to make, in conjunction with experimental lots of the dehydrated medium, a supply of tellurite solution which I have titrated.

After exactly 24 hours of incubation on this medium at 37° C. (some minute colonies of *Streptococcus mitis* may be more easily distinguishable with longer incubation), *S. salivarius* produces blue smooth or rough "gum drop" colonies from 1 to 5 mm. in diameter which can be easily recognized by reflected light. *S. mitis* produces small blue colonies 0.5 mm. or less in diameter which are slightly raised. Enterococci produce dark blue or black shiny, slightly raised colonies, 1 to 2 mm. in diameter, which have a characteristic appearance and are easily differentiated from *S. salivarius* and *S. mitis*, particularly when viewed by reflected light. Other types of streptococci, which are uncommon, have not been studied. Coliforms may grow occasionally, but they produce brown colonies and rarely spread. The pleuropneumonia group produces mucoid colorless colonies from 5 to 10 mm. and more in diameter. Molds grow after two days.

In vitro *Testing of Toxigenicity of Streptococci.* This test of the toxigenicity of streptococci is based on the facts that (1) toxigenic bacteria resist the antibacterial action of fresh, diluted, defibrinated guinea pig blood, [32-47] and (2) streptococci that resist the antibacterial power of the fresh guinea pig blood also resist appropriate time/concentrations of a number of injurious agents.[16] These relationships have been substantiated by innumerable subsequent experiments in this laboratory and elsewhere.

The method used to test streptococci is as follows:—Cut 10 *S. salivarius* and 10 *S. mitis* colonies from the medium, using an inoculating needle which has been flattened at the tip like a spear. Shake each colony and the attached medium into a 100 x 25 mm. test tube containing 10 ml. of brain heart infusion (Difco). Place the tubes in the incubator. *S. salivarius* begins to produce turbidity or sediment in 6 hours and *S. mitis* may begin to show growth in about 12 hours. After this initial period, take the tubes from the incubator every hour and inspect them for evidence of growth. Remove those that are sufficiently cloudy or have a sufficient amount of sediment, place them in the refrigerator, and test as follows as soon as possible afterwards: If the growth occurs almost entirely as a sediment, decant enough supernatant to make the resulting suspension approximately of standard density, which is easily recognized by the experienced worker. Mix a loopful of the growth with 0.5 ml. of 1:125,000 hexylresorcinol. If the amount of growth is too small, use a drop instead of a loopful of culture. Streak a loopful of the resulting suspension halfway across a blood agar plate to serve as a control of the density of the growth. Make 10 to 12 streaks on each plate. Immediately incubate the suspensions for exactly 110 minutes, timing them from the start of each plate and then streak a second loopful, making it continuous with the corresponding control streak to permit easy comparison. Discard the tubes and incubate the blood agar plates exactly 24 hours, not "overnight." Observe any hemolytic growths and any evidence of impurity of the growths and compare the density of growths. Those streaks that show as dense a growth in the second streak as in the control are toxigenic.

Distribution of Number and Proportion of Toxigenic Streptococcus salivarius *and* Streptococcus mitis *in Man.* The nasal cavities should contain no toxigenic streptococci, but the properly obtained (*i.e., thoroughly* swabbed, particularly behind posterior pillars and any hyperemic areas) pharyngeal swab contains them in proportion to the extent of local intoxication, which may not, however, coincide with the appearance of the membrane. Normally, a properly obtained throat swab contains less than 200,000 total streptococci and less than 20 per cent of them are toxigenic. *Streptococcus salivarius* and *S. mitis* are present in approximately equal number and proportion in most instances. On the contrary, gingival crevices or pockets and extracted teeth contain practically no S. *salivarius.* In the absence of gingivitis, a gingival swab contains about as many *S. mitis* as does a throat swab of the same person, and the proportion of toxigenic colonies is also similar. Non-infected teeth, smashed on a steril anvil, rarely contain more than 1,000 total streptococci and less than 20 per cent of them are toxigenic. When the dental and periodontal tissues are infected, the number and proportion of toxigenic streptococci rise in proportion to the extent and severity, respectively, of the toxic infection.[5,22,31] Contrary to expectation, the bulk of the dental infection in teeth without peridentitis is in the chamber.[5]

When toxigenic streptococci are swallowed and the gastric acidity is within normal limits, the toxic products inhibit proliferation of *Escherichia* and *Aerobacter* and reduce the lactose fermenting power of these organisms in the gastrointestinal tract.[15,18] However, in the absence of gastric HCl, there is nothing to check the coliforms initially, and they are then able to proliferate rapidly, regardless of the presence of streptococcus toxins.[15,21] This relationship is so constant that the fecal coliform count can be used as an indicator of achlorhydria.

These relationships apply to semisolid specimens obtained after a laxative or from persons with diarrhea. In the formed stool, there is a considerable increase in coliforms and enterococci and a marked decrease in streptococci. In semisolid feces, however, and with normal gastric acidity, the greater the streptococcal intoxication the fewer the coliforms (and the more paracoli). The number of fecal bacteria is best expressed in millions per 100 grams of dry feces. The normal stool contains about 100,000 coliforms, but the toxic stool may contain less than 1,000 and in achlorhydria the number rises above 1,000,000.

The number of streptococci in the semisolid fecal specimen probably represents the ability to multiply in the gastrointestinal tract as a result of being able to overcome local defensive measures, thus making the total number of streptococci a rough index of their pathogenicity, as in other parts of the body. However, in highly toxic stools, streptococci may be inhibited and may even disappear. The proportion of toxigenic variants, on the other hand, represents the severity of the intoxication.

Hematological Considerations. Better understanding of hematological relationships of streptococci in man depends, above all, on a more accurate and more significant hematological technique than is generally used. The leucocyte count pipette must be mechanically shaken two minutes and two drops must be counted. If they disagree, further counts must be made. The film must be spread rapidly immediately after the drop of blood has been obtained. It must be thin enough so that, in a considerable portion, the leucocytes are well isolated and sufficiently spread out to reveal the finer nuclear structure clearly. The dye solution must be made from a Commission Certified or other satisfactory batch of dye, suitably diluted so as to preserve its staining properties; the proportion between dye and buffer diluent must be 1:2 (by volume, not the number of drops); and the timing must be adjusted to give best results. At least 200 cells must be counted and differentiation of maturity from immaturity made on the basis of the presence or absence, respectively, of a filament, not a narrow band. The nonfilament-filament ratio is expressed in per cent.[23] This technique permits better understanding of hematological relationships, and the normal range is much narrower than the present arbitrary one. Experience in this laboratory indicates that, instead of 5,000 to 10,000 per cu.mm., the normal total leucocyte count is from 7,800 to 8,900; the polymorphonuclears, from 60 to 66 per cent; the mature polymorphonuclears, from 45 to 55 per cent; and the nonfilament-filament ratio, from 15 to 22 per cent. These values appear to be borne out by the effect of leucocyte-stimulating agents,[49] the total number of mature leucocytes being inversely proportional to the amount of leucocyte-stimulating agent required to bring the amounts within the newly established normal limits.

In a few, mostly early cases, the values are above normal due to stimulation of leucopoiesis, but in most persons with low-grade chronic infection the toxins have acted so long that they have had an unfavorable effect on leucopoiesis and the values are subnormal. Usually, the greater the streptococcal intoxication, the lower the hematologic values. The relationship is quite close with streptococci but not with other bacteria.

Some persons show inhibition of some cell types but normal or even stimulated values in others, making it difficult to appraise the results. More recent investigation has shown the extreme importance of the mature polymorphonuclear neutrophilic leucocyte (the "fighter"). The proportion may not always reflect the true degree of toxic effect, but this defect has been largely overcome by calculating the total number of mature leucocytes. While this requires unusual technical skill, the results are well worth it. The total number of mature leucocytes in non-infected persons and those with no toxic inhibition of leucopoiesis is 3,600 per cu.mm., but about 90 per cent of persons with low-grade chronic infection show values below this figure, the depression being proportional to the loss in defense against infection. When the number of total leucocytes is above normal, the total number of mature leucocytes should be proportionately higher or the leucocytosis will be inefficient.

Though the blood sedimentation rate is less constantly related to the number and proportion of toxigenic streptococci, such a relationship actually does exist.[23]

The bacteriological and hematological values follow a seasonal trend with peaks in May and December and troughs in February and August. Likewise, there may be marked differences from the average trend at any particular time, due to inadequately understood climatic, epidemic, and other conditions.[50-52]

Realizing the many shortcomings of the methods, it is possible to allocate findings in different degrees of infection or intoxication, as shown in TABLE 1.

TABLE 1

RELATION OF DEGREES OF INFECTION OR INTOXICATION TO THE DIFFERENT VALUES DISCUSSED

	Relatively non-infected persons	Mild infection or intoxication	Moderately severe infection or intoxication	Severe infection or intoxication
Total number of streptococci:				
Pharynx	Less than 200,000	200,000 to 1,000,000	1,000,000 to 20,000,000	Over 20,000,000
Gingivae *(S. mitis)*	Same as the pharynx	2 to 3 times the pharynx	4 to 10 times the pharynx	More than 10 times the pharynx
Extracted teeth *(S. mitis)*	1,000	1,000 to 50,000	50,000 to 500,000	Over 500,000
Feces: intox.	Over 1,000	200 to 1,000	50 to 200	Less than 50
infection	Less than 50	50 to 200	200 to 1,000	Over 1,000
Proportion of toxigenic streptococci:				
Pharynx	Less than 20%	25 to 50%	50 to 80%	Over 80%
Gingivae *(S. mitis)*	Same as the pharynx	10% more than the pharynx	20% more than the pharynx	Over 30% more than the pharynx
Extracted teeth *(S. mitis)*	Less than 20%	25 to 50%	50 to 80%	Over 80%
Feces	Less than 25%	25 to 50%	50 to 80%	Over 80%
Coliforms in feces:				
In achlorhydria	100,000	The number is above 1,000,000, may rise to 20,000,000		
Otherwise	100,000	40,000 to 90,000	5,000 to 40,000	Less than 5,000
Paracoli	None	Number usually increases with intoxication giving a high paracoli: coli ratio.		
Sedimentation rate (Westergren), mm in 1 hour (fresh blood):				
	Less than 5	5 to 20	20 to 50	Over 50
Stimulation of leucocytes:				
Total WBC	7,500-8,900	9,000-10,000	10,000-12,000	Over 12,000
Nonfil.-fil. ratio	20%	20-30%	30-75%	Over 80%
Polys.	60-66%	68-70%	70-75%	Over 75%
Mature polys.	45-55%	These are rarely over 55%, even in acute infections		
		The higher the percentage, the more efficient the leucocytosis		
Monocytes	2-4%	4-5%	5-7%	Over 7%
Eosinophiles	2-3%	3-4%. Higher values are more characteristic of allergy		
Depression of leucocytes:				
Total WBC	7,500-8,900	6,000-7,500	4,500-6,000	Below 4,500
Nonfil.-fil. ratio	20%	Values usually increased, but often rise with effective response, indicating that apparently low values were actually depressed values.		Below 20%
Polys.	60-66%	52-60%	45-52%	Below 45%
Mature polys.	45-55%	40-45%	30-40%	Below 30%
Total mature polys.	3,600	2,500-3,500	1,800-2,400	Below 1,800
Monocytes	2-4%	2%	1-1 5%	Below 1%
Eosinophiles	2-3%	1%	0.5%	None
Bacterial products in urine (units):				
	Less than 5	6 to 15	16 to 30	Over 30

Lab. number
Patient
Physician

Date begun

HEMATOLOGY

		Line	"Normal" range[1]	Findings in the average chronic invalid[2]: Low	Medium	High
Leucocytes: Total	per cu. mm.	1	7,500 to 8,800	5,900	7,600	9,500
Total mature	per cu. mm.					
Neutrophiles,						
filamented (mature)	%	2	50 to 6		34	
nonfilamented	%	3	7		20	
total	%	4	60 to 65	45	54	62
nonfilament-filament ratio	%	5	15[3]	40	60	91
Lymphocytes	%	6	30 to 40		38	
Monocytes	%	7	2 to 4	3.4	5.1	7.6
Eosinophiles	%	8	2 to 3	1.0	2.4	4.5
Basophiles	%	9	0.5	0.3	0.7	1.3
Blood sedimentation rate (Westergren)	mm. in 1 hour	10	less than 5	9	20	39

BACTERIOLOGY

Microorganism[9]	Total number[4,5]	Line		Percentage of toxigenic colonies[6]	
NASAL CAVITIES					
Streptococcus salivarius[7]		11	none		
mitis		12	none		
hemolyticus		13	none		
Pathogenic staphylococci[8]		14	none		
Other bacteria		15	none		
THROAT					
Streptococcus salivarius		16	less than 250,000 and less than 20% of each		40,000,000 and 67%
mitis		17			25,000000 and 62%
hemolyticus		18	none		
Pathogenic staphylococci		19	none		
Other bacteria		20	none		
GINGIVAL CREVICES OR POCKETS					
Streptococcus salivarius		21	less than 50,000 and less than 20%		50,000 and 18%
mitis		22	less than 250,000 and than 20%		same as in the pharynx of the person
hemolyticus		23	none		
Pathogenic staphylococci		24	none		
Other bacteria		25	none		
TOOTH					
Streptococcus salivarius		26	none		
mitis		27	less than 1,000 and less than 20%		
hemolyticus		28	none		
Pathogenic staphylococci		29	none		
Other bacteria		30	none		
FECES					
Escherichia (*B. coli*) and *Aerobacter*		36	75,000 to 250,000		
Paracoli (lactose nonfermenters)		37	none		
Enterococci		38			
Streptococcus salivarius		39	none		
mitis		40	none		
hemolyticus		41	none		
Pathogenic staphylococci		42	none		

CHART 1 (*see explanation on page 52*).

REMARKS

Low counts indicate that toxins are preventing leucocytic response, which is characteristic of severe intoxication and low resistance, whereas high counts indicate active response to the infectious agent. Since these are the "fighters," the percentage is proportionate to the defense against infection.

Same as line 1.
Except in severe infections with poor resistance, which produce a low ratio, the ratio generally is proportionate to the severity of the infection.

Same as line 1.
Same as line 1. Eosinophilia may be associated with hypersensitivity to the infection.

Usually, the rate is proportionate to the severity, extent and duration of the infection.

Nasal streptococci and staphylococci may be caused by diffuse inflammation of the lining membranes or may be draining from the sinuses.

Same as line 20.

The higher the number of streptococci the more sure and more extensive the infection. The higher the percentage of toxigenic colonies, the more severe the intoxication.

Pathogenic staphylococci have local rather than general significance.
Other bacteria, usually coliforms, occur when the infection is severe.

This occurs in small numbers in the gingival margins, but may increase when local resistance is low.
Normally, the number and percentage of *S. mitis* are similar to those in the throat. The higher the gingival counts, the surer and more severe the gingivitis.
Rarely found in gingival crevices.
Same as line 20.
Same as line 20.

This is uncommonly found in aseptically extracted teeth.
The higher the number and percentage of toxigenic *S. mitis*, the more severe and more extensive the intoxication in the tooth. Teeth with counts above 1,000,000 are likely to be major foci of infection.
Hemolytic streptococci are rare in teeth.
Same as line 20.
Same as line 20.

In achlorhydria, the count is above 1 million. Otherwise, the lower the count, the more severe the intoxication.
In severe intoxications, there may be almost complete absence of all types of bacteria.
Paracoli are characteristic of severe intoxication. Usually, the higher the number the more severe it is.
Enterococci are nonpathogenic in the majority of cases.
The percentage of toxigenic streptococci is proportionate to the severity of the intoxication. When the intoxication is caused by swallowed toxins, there may be few streptococci in the feces. An increase in the fecal streptococci suggests actual invasion of the intestinal mucosa.
Usually residual from the upper respiratory tract, but often contribute to intestinal symptoms.

CHART 1 (*continued*).

Excreted Bacterial Products. The author has devised a method of titrating certain bacterial products in urine that takes about 30 seconds and requires only 1.0 ml. of sample. To do the test, add 10 per cent Roccal drop by drop to 1.0 ml. of urine until the precipitate which increases and then decreases in turbidity shows no further decrease. The end-point is an opalescence in most cases. Each 0.01 ml. of reagent required is considered equivalent to 1 unit of bacterial products. Persons with little bacteriological and hematological evidence of infection show less than 5 units, but in severe infections the value may rise above 100.

Theoretically, when streptococci multiply in the relatively immune person, the metabolic products are rendered innocuous by the defense mechanism, but, when the effectiveness of the defense mechanism has been reduced, some of the bacterial products are unaffected and are excreted unchanged in the urine. This hypothesis is confirmed by the new titration method. When the mature leucocyte count is normal or above normal (*i.e.,* 3,600 or more per cu.mm.), the amount of titrated bacterial products in the urine is low, whereas when the number of mature leucocytes is low the amount of bacterial products in the urine is much higher, usually in proportion to the extent of depression of the total number of mature leucocytes. Hence, the titer appears to indicate the amount of bacterial products that the body has been unable to destroy and, if so, is another measure of the defense mechanism. The method has been used successfully in following response to treatment of the infection.

This method is suggested here (as a preliminary announcement) only because of its connection with the other subjects discussed and because it is hoped that it will stimulate others to study what appears to be a rapid, simple method easily adaptable to bedside technique. At present, no claim is made as to the type of bacterial product which it measures, nor as to the specificity. These phases, along with study of other high molecule agents of a similar type, will be discussed elsewhere.

Explanation of superscripts in CHART 1 (*see pages* 50–51).

1. Theoretically, a normal person is free from infection, but this rarely occurs and the "normal" person is usually considered one without severe infection. The normal values here, however, are those of persons who have negligible infection.
2. The bacteriology, hematology, and severity of infection are subject to considerable seasonal variation. Usually, the values are comparatively mild in February and August and severe in May and December, but they may differ from this pattern.
3. Although the "normal" nonfilament-filament ratio (the Schilling index) is considered to be 6 per cent, very few people show such a low value. Our average in relatively non-infected persons is about 20 per cent and values below this indicate that the toxins have prevented the leucopoietic tissues from responding to the toxic stimuli.
4. Special methods have been developed in this laboratory for isolating pathogenic types of staphylococci and streptococci. They permit reliable isolation even when numerous other bacteria are present. Hence, the proportion of positive findings is much higher than elsewhere.
5. Fecal bacteria are counted on the basis of millions per 100 grams of dry feces Other foci are estimated on the basis of the total number in the swab or tooth. Teeth are broken aseptically. Experiments show conclusively that in carious teeth the bulk of the intoxication originates in the streptococci in the cavity, even when the x-ray indicates periapical involvement In such cases, there are very few toxigenic streptococci around the apex of the tooth, but large numbers in the chamber. When there is extensive involvement of the root canal, the root fragments also show a high count Non-vital teeth usually show higher counts than do vital teeth, but since the number and percentage of toxigenic streptococci is proportionate to the extent and severity of decay, the relation to vitality is only a general one.

6 Toxigenic streptococci, the common denominator of chronic illness, produce a toxic principle which is distinct from other pathogenic factors and may be readily demonstrated by an appropriate technique. About 20 colonies of streptococci are tested from each sample. The percentage of toxigenic colonies usually is proportional to the severity of the infection in the part tested. These toxigenic streptococci make excellent antigens, whereas nontoxic strains have little value. The resistance of streptococci to certain injurious agents has been shown to be parallel with the toxigenicity of the cultures. Its precise determination depends upon careful attention to numerous technical details.

7. The old classification of nonhemolytic streptococci as "*viridans*" and "nonhemolytic" is inaccurate. The differentiation is now based on careful and extensive study of biochemical properties. Generally, "nonhemolytic" streptococci are *Streptococcus salivarius* and "*Streptococcus viridans*" are *Streptococcus mitis.*

8 Staphylococci that coagulate blood are probably pathogenic Others are ignored except in special cases.

9. Most non-pathogenic bacteria or those of little pathologic significance are ignored.

Summary of Research Project: Distribution, extent, and toxicity of streptococci and distribution of pathogenic type staphylococci in possible foci of infection.

Date of report

Patient

Physician

HEMATOLOGIC FINDINGS

	Stimulated response (degree above normal)	Toxic inhibition (degree below normal)
Nonfilament-filament ratio		
Total leucocytes		
Neutrophiles		
Mature neutrophiles, per cent		
Total mature neutrophiles		
Monocytes		
Eosinophiles		
Acceleration of blood sedimentation rate		
Urine polysaccharide titer		

BACTERIOLOGIC FINDINGS

	STREPTOCOCCAL TOXIN			Pathogenic type staphylococci
	Extent (based on relative number)	Intensity (based on relative percentage of toxigenic)	Intensity (based on coliforms)	
Nose				
Throat				
Feces				

CHART 2

Reporting the Results of the Tests. Although numerous different report forms have been worked out from time to time, it is difficult to include all necessary data and retain simplicity. More recently, the detailed report form (CHART 1) has been accompanied by a Summary of Research Project form (CHART 2). The forms are reproduced here, therefore, not with any dogmatic finality but with the idea of pointing the way to clearer reporting of the different findings.

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Note. The Section of Psychology held a meeting on November 17, 1947, at which Doctor Gustave Mark Gilbert, Metropolitan State Hospital, Waltham, Mass., presented a paper entitled, *An Analysis of the Psychological Examinations Administered to the Nazi Leaders at Nuremberg*. No abstract of this has been received.

The Section of Anthropology held a meeting on November 24, 1947, at which Mr. Harry Tschopik, American Museum of Natural History, New York, N. Y., presented a paper entitled, *Chucuito: an Aymara Indian Community in Southern Peru,* illustrated by motion pictures and recorded music. This will be published in a later issue of the present volume of TRANSACTIONS.

SECTION OF PHYSICS AND CHEMISTRY

ADSORPTION AS A RESEARCH TOOL

By A. C. ZETTLEMOYER*

During the past ten years, extensive developments have been made in the application of gas adsorption techniques to the solution of research problems. This period began with the work of Brunauer, Emmett, and Teller, who for the first time devised a method for determining the surface area of a solid with considerable precision and accuracy. At the present time, the physical and chemical structure of the surface can often be elucidated by gas adsorption studies. If the solid material is non-porous, average particle size can also be determined. Due to these developments and to the importance of a knowledge of the properties of interfaces in many technological developments, many companies in recent years have added gas adsorption research groups to their research laboratories. Today, gas adsorption techniques are used to study adsorbents, catalysts, pigments and, indeed, any solid for which interaction with gases or liquids is the important phenomenon.

One of three types of gas adsorption apparatus is usually employed. The Emmett and Brunauer apparatus is the most common. For surface area measurements, nitrogen is used as the adsorbate and the adsorbent is held at the temperature of liquid nitrogen. The nitrogen gas is introduced from a burette through a dead space kept as small as possible to minimize the dead space correction. Helium is used to calibrate the dead space; this gas is not adsorbed to any appreciable amount at the temperature of liquid nitrogen. The temperature of the bath is usually measured with a nitrogen vapor pressure thermometer. This type of apparatus possesses the disadvantage that the equilibrium points are approached from pressures above the equilibrium pressure going up the isotherm and from pressures below going down. Nitrogen is used because it is a small molecule, is inert, and may be readily obtained in a pure state.

The McBain-Baker balance is used for studying the adsorption of organic vapors and for high-temperature adsorption studies. This apparatus makes use of a quartz spiral spring to hold the small bucket containing the sample. As the weight of the sample increases due to adsorption, the extension of the spring is followed with a cathetometer. No dead space correction is necessary. An interesting modification of this apparatus, using a beam instead of a spring, has recently been devised by Gregg.

The Wooten-Brown apparatus is used for the measurement of solids having areas below one square meter per gram. For such low areas, the amount adsorbed will be relatively small. Therefore, the dead space correction is greatly reduced by using a gas far below its condensation pressure; for example, krypton may be used at the temperature of liquid nitrogen. Then, even at high relative pressures, very little gas will be in the dead space. Mercury cutoffs must be used instead of stopcocks and pressures are read on a McLeod gauge.

A number of interesting problems with regard to pigments, adsorbents, and chemical reagents have been solved in the Lehigh Gas Adsorption Laboratory. An especially interesting one concerns yellow iron oxide. When this monohydrate is prepared by the corrosion process in the presence of certain impurities, a richer-looking product is produced than when these impurities are not present. This pigment forms a transparent coating when dispersed in linseed oil. While usual physical tests such as x-ray failed to distinguish the new variety from the old, differences could easily be distinguished by gas adsorption technique. The surface area of the new type was found to be about ten times greater than that of the older type and the pore size of the new was found to average only one-half that of the old.

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* William H. Chandler Chemistry Laboratory, Lehigh University, Bethlehem, Pa. This paper, illustrated by lantern slides, was presented at the meeting of the Section on November 28, 1947.

Another problem involved a series of nine manganese dioxide ores which were examined for differences in surface area. These materials showed a wide difference in activity when used in dry cells. The surface areas differed in the same order. Thus, a technique for predicting their activity was worked out. Activity, of course, is not necessarily dependent on surface area but depends also on the nature of the surface. It is activity per unit area which is the more basic factor. In the case of the dioxide samples, as often happens, the extent of the surface is the important factor.

Recently, we have concerned ourselves with adsorption on organic surfaces. This field has been little explored but offers excellent possibilities for the use of the adsorption technique. The work of Pauling on the structure of proteins, for example, has indicated that the surface structure is the controlling factor in deciding the specificity of the activity of the protein. While some work has been done in our laboratory on gas adsorption on collagen, it appears necessary to initiate a systematic study of adsorption on organic surfaces. This must necessarily begin with studies of physical adsorption on pure hydrocarbon surfaces. This work has just gotten under way.

Much of the work in the Lehigh Adsorption Laboratory has been and is concerned with Active Magnesia, work done under the auspices of the Westvaco Chlorine Products Corporation. We started work on this material several years ago when it was used in making 1707 catalyst for the synthetic rubber program. Low-temperature nitrogen adsorption isotherms were measured on a series of commercial active magnesias. The isotherms were mixtures of types II and IV. The isotherm shape was explained by hypothesizing a structure consisting of plates of checkerwork of cubic holes and crystallites. The surface areas of the magnesias were shown to range from less than 1 to more than 200 square meters per gram. Some difficulty was encountered in the use of the usual methods for calculating areas from adsorption data. The areas were shown to be directly proportional to the amount of iodine adsorbed from solution, or the iodine number.

Active Magnesia is now used as an adsorbent in removing silica from boiler water, fatty acids from petroleum ether, and impurities from petroleum lubricants, dry cleaning solvents and other non-aqueous and unreactive fluids, and in chromatography. Active Magnesia shows good promise also as a basic, solid catalyst for vapor phase organic reactions.

Because the BET plots for the active magnesias were always found to be slightly concave to the pressure axis, there was some concern as to whether the BET theory could actually be applied. Modifications of the BET theory which were in the literature were investigated without success. A dual surface adsorption theory based on the multi-molecular theory of Brunauer, Emmett, and Teller was found to explain adequately the adsorption isotherms. This theory involves the concept that commercial magnesium surfaces are made up of two portions differing in their heats of adsorption. The one portion consisting of about 25 per cent of surface has a very low heat of adsorption for nitrogen, and this means that the c value connected to the BET theory is very low for this portion.

Since magnesium oxide has a regular cubic lattice made up of magnesium and oxide, it is very adaptable to a precise study of adsorption. The 100 face of the crystal is the one most always found. Therefore, a number of precise studies have been made of the physical adsorption of gases on pure magnesium oxide.

Physical adsorption of gases on solids is generally considered to take place with a close-packed first layer. Using this concept, the area occupied per molecule can be calculated from the density of the normal condensed phase. Areas calculated accordingly from the adsorption of various adsorbates on active magnesias were non-concordant.

To explain these discrepancies, the implications of adsorption in preferred positions were examined. From the geometry of the 100 face of a magnesium oxide crystal, distances between preferred positions were determined. Eigenvalues for the area per adsorbed molecule could then be calculated as a function of these distances and the size of adsorbate molecules. These eigenvalues for the effective molecular areas are simple multiples of a constant characteristic of the crystal face. This relationship leads to the conclusion that, when adsorption takes place in preferred positions, the ratio of the moles of adsorbate A to the moles of adsorbate B required to complete a unimolecular layer on the surface can be expressed in terms of simple whole numbers.

Data for the adsorption of a number of gases and for adsorption of iodine from solution on chemically proven magnesium oxide were found to be in excellent agreement with this postulate.

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Elected November 20, 1947

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SECTION OF GEOLOGY AND MINERALOGY

MARINE SEDIMENTS AND PLEISTOCENE CHRONOLOGY

By WILLIAM D. URRY[*]

The sediments covering the floor of the great ocean basins, beyond the continental shelves, are accumulating at an exceedingly slow rate. Consequently, a few feet of these deposits often provide a continuous record of the history of terrestrial events that have occurred within the last million years. Under special conditions, the period may be much longer, extending perhaps over a considerable portion of Tertiary time, though the record is likely to be discontinuous.

Several ingenious methods[1] of securing vertical sections of the abyssal sediments, in the form of cores, are in general use. Examination of many such cores reveals the fact that, probably in the majority of cases, an excellent record of earth history is preserved undisturbed by the many processes that obliterate the details of similar records on land. There seems to be no reason why an intensive study of the ocean floor should not yield the full chronological story of the glacial period of the Pleistocene, the most recent period of the earth's history, but one still full of unsolved and intriguing problems.

Concerning the last glacial stage, and its sub-stages—the Wisconsin of North America—the geologist has amassed a considerable body of facts, largely because it was an event of but yesterday reckoned in geological time. Nevertheless, the estimates of the chronology of even such recent events are confusing, at least in North America. This has serious repercussions in other fields of study, as, for example, in the story of man in the Western Hemisphere. Prior to the last glacial stage, the earth was subjected to great advances of ice on at least three consecutive occasions. Concerning the details, particularly the chronology, of these earlier glacial stages and the intervening warm spells, much of our knowledge is conjectural.

While the principal purpose of this presentation is to draw attention to a method of measuring time in the ocean sediments, any description would offer barren reading without reference to the methods, results, and conclusions of researchers in several branches of the geologic sciences, whose studies are, of course, the very essence of the problem in that they provide the evidence of changing conditions in the deposition of the sediments. There would be little incentive to measuring time in a sediment if it exhibited no distinctive changes in the past. Fortunately, nearly all cores of deep-ocean sediments are truly rich in events.

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[*] Geophysical Laboratory, Carnegie Institution of Washington, Washington, D. C. This lecture, illustrated by lantern slides, was presented at the meeting of the Section on December 1, 1947.

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Often, as in the suite of North Atlantic cores secured by Doctor C. S. Piggot,[2] the lithologist is able to distinguish buried zones of glacial marine débris interbedded with oozes characteristic of warm surface waters, or zones in which, for example, the incorporation of volcanic glass shards is a distinctive feature. Lacking any such immediately apparent evidence of changing conditions, it may still be possible to obtain a detailed picture of past climatic changes, as in sediments consisting almost wholly of foraminifera and cementing clay. Doctor J. A. Cushman's work[3] on the globigerina oozes of the Caribbean Sea is an excellent example. Studies of the faunal distribution of many other types of fossils in the ocean bottom have also proved highly productive of evidence for changes in the past.

The comprehensive studies of deep-sea cores[4] performed and documented by members and co-workers of the United States Geological Survey constitute an excellent pattern for guiding future work; and, highly detailed as these studies may appear to be to the reader, much of the spade work cannot be neglected if we are to benefit fully from the wealth of information available at the bottom of the ocean. These researches standing alone are highly important, but they acquire added significance if a sound basis can be established for correlation from place to place, from land mass to land mass, from hemisphere to hemisphere. Most assuredly, this correlation cannot be based on a spatial dimension such as depth, any more than such a yard-stick is of use on land. This is quite obvious from an examination of the lithology of cores from even a very limited area like the western basin of the North Atlantic.

The major problems of the Pleistocene necessarily involve all the great ocean basins. The only dimension that can form a true basis for correlation is that of absolute time. Estimates of the rate of deposition, determined by subjective methods, have appeared in the literature[5] and are in substantial agreement with rates determined by the presently described objective procedure in the order of magnitude of the grand averages. Nevertheless, the rate of deposition has been so variable from place to place and time to time that nothing but confusion could result from using such results to date a specific event in the sediment.

As is the case in many researches, the initial results which pointed the way to a method of measuring time in the ocean sediments were incidental to the problem. Piggot and Urry[6] were searching for an explanation of certain phenomenally high radium contents in grab samples collected on the last cruise of the *Carnegie*.[7] To this end, the radium content of small in-

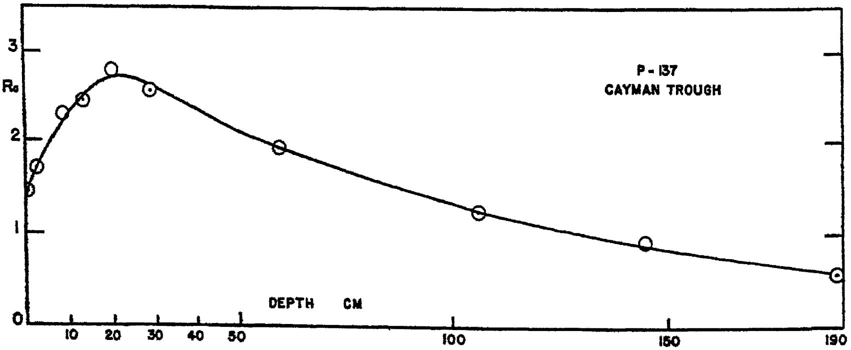

FIGURE 1. A typical curve of the radium content in 10^{-12} gram per gram plotted as a function of the depth in a core of deep-sea sediment.

crements of deep-sea cores was being measured. FIGURE 1 illustrates a curve of the radium content as a function of depth in a core that has proved to be typical of the sediments in some fifteen cores from the Ross Sea to the North Pacific and from the Caribbean to the North Atlantic. Naturally, there are minor variations that depend on the type of sediment and particularly on the span of time represented by the core.[8] It is now established that the high radium content in the surface of many deep-ocean sediments is a transitory feature. There appears to be only one plausible explanation of such a specific picture of the variation of radium content with depth in the deposits of the deep ocean.

The peculiar features of a maximum and the eventual establishment of a low and constant radium content[9] (not to be seen in FIGURE 1 but observed in the lower half of several cores from the Pacific Ocean) receive a complete explanation if, during the course of their deposition, the sediments exhibit a preference for certain members of the uranium family.

In old radioactive minerals, the relative amounts of the various members do not change with time; they are in radioactive equilibrium. Were one to save all the separations of a chemist's analysis of an old pitchblende and request posterity to study the portions from time to time for the next half-million years, the final result would be characteristic of what one finds in the ocean sediments, particularly for the rare-earth fraction which would contain ionium, the immediate parent of radium. The original radium would have been separated from its parent but new radium would accumulate in the ionium fraction until, in 10,000 years, radium and ionium would again be in equilibrium. Thereafter, the radium content of the ionium fraction would decrease, but only so fast as its parent decays, which is at the rate of one-half every 82,000 years. It is quite practical to follow such a decrease for five half-lives, even with extremely small concentrations, and thus posterity would have a clock that would not run down for close to half a million years. In studying the uranium fraction of the chemist's separation, it would be found that the ionium content, and with it the radium content, was continuously increasing until after half a million years no further change occurred. Uranium decays so slowly and is present in such prodigious amounts compared with its daughter elements that the supply is virtually inexhaustible for our purpose.

In the ocean bottom, nature is a poor substitute for the chemist and the separations are only partial. The sediments exhibit a decided preference for ionium and are non-committal about radium, while showing a great but not complete dislike for uranium. It is on account of the incompleteness of this dislike that the radium content of the ocean sediment does not decrease to zero, and this property is fully utilized in determining the uranium content from the radium measurement wherever the sediment represents a span of time greater than half a million years.

Such a theory to account for all the details of the variation of the radium content in the bottom sediments receives substantial support in studies of the radioactivity of sea water.[6] Here, one finds the exact counterpart of the condition of deficiency of uranium with respect to equilibrium with radium (and ionium) obtaining in the surface of the ocean bottom: the ocean waters possess a uranium content greatly in excess of that necessary to support the radium through its parent, ionium.

Because of the partial nature of the separations of the radioactive elements in the surface of the sediments, the mathematically possible paths by which the radium content can reach a state of equilibrium are many and tortuous. Nevertheless, with the aid of the equations governing the growth and decay of the radioactive elements, it is relatively easy to follow them all.[10]

It is apparent, then, that the deep-ocean sediments possess a built-in clock which we are at liberty to employ with one further assumption, namely, that the sediment at the location of any given core has always acquired, at the time of its deposition, a constant amount of the radioactive elements. This appears to be a drastic assumption, but there is much evidence to support it. For example, measurements of the uranium content made independently of the radium determinations have shown a constant uranium content throughout a core—a core that exhibited a variation of the radium similar to that shown in FIGURE 1. Furthermore, the flat tails of five curves of radium content against depth show no tendency to wiggle, indicating that the uranium content acquired over long periods, as much as 500,000 years in one case, was very constant. This latter analysis can, of course, be made only when the sediment represents a span of time greatly in excess of 400,000 years.

Application of this clock to the dating of events in marine sediments has, as yet, produced little more than the signs pointing to the possibilities. In the North Atlantic, where rates of deposition have been high, a core ten feet in length covers little more than the Wisconsin Glacial Stage. It has proved possible to date, and to correlate on this basis, the changes in the sediments of the North Atlantic which represent the equivalent of the four substages of the Wisconsin ice on land. It also has been possible to establish a chronology of the climatic changes that occurred during and following the glacial sub-stages and to date the "climatic optimum" of post-glacial time as indicated by the relative abundances of various types of foraminifera. Many of the changes associated with the North Atlantic sediments proved to be contemporaneous with effects arising from a common cause to be expected on land, provided the estimates of the subjective methods of measuring time in glaciated regions are accepted.

While the longer cores from the North Atlantic reached back no more than 70,000 years, even short cores from the Bartlett Deep in the Caribbean Sea were found to represent a span of 200,000 to 300,000 years. The relative abundances of the foraminifera in two such cores, which presumably indicate the climatic changes at the surface of the Caribbean Sea, exhibit a good correlation based on the dating but no correlation on the basis of depth because of the different rates of deposition. Both cores show that the periods of warm and cold surface waters are contemporaneous with the rise and fall of the snow-line level in the Alps, although no detailed comparison of the chronology of the European and North American glaciation is possible as yet. A detailed analysis of the foraminifera near the top of one core from the Caribbean Sea was completed, and periods of cold and warm surface water appear to exhibit a chronological correspondence with the alternating zones of glacial marine débris and warm water deposits in the dated cores of the North Atlantic. A full discussion of these results and an acknowledgment of the cooperative efforts of many researchers have already been published.[8]

From these beginnings, it appears that the effects of glaciation over widely separated areas of the northern hemisphere are roughly contemporaneous. Our present knowledge of the chronology of the effects of glaciation in the southern hemisphere, particularly with reference to the date of any specific event in the northern hemisphere, is purely conjectural. Very recently, curves of the radium content as a function of depth have been constructed for several cores collected by Dr. J. L. Hough on board the U.S.C.G.C. *Northwind* with the Navy Antarctic Expedition of 1946-47. These cores are all from the southern hemisphere from just south of the

Equator to the Ross Sea. With the completion of the necessary lithological and foraminiferal studies, it should be possible, in the near future, to provide a preliminary answer to the outstanding question of glaciology—Is the glaciation contemporaneous in the northern and southern hemispheres?

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SECTION OF BIOLOGY

December 8, 1947

SYMPOSIUM: EFFECT OF DERIVATIVES OF FOLIC ACID ON CERTAIN TYPES OF NEOPLASTIC DISEASE

INTRODUCTION TO THE CHEMISTRY AND ACTION OF CERTAIN PTEROIC ACID DERIVATIVES

By COY W. WALLER*

On May 29, 1946, a series of papers[1] was presented before this Academy on the isolation, characterization, synthesis, and action of pteroylglutamic acid. The genesis of pteroylglutamic acid has been reviewed[2] and will not necessarily be given here; however, the origin of the chemical work presented at that meeting was also the beginning of the work to be presented at this time.

In 1942, when Dr. Brian L. Hutchings began work at the Lederle Laboratories, the investigations on folic acid in a number of widely separated fields were converging. Dr. Hutchings began by making a concentrated effort to find new sources for folic acid materials which would result in increased yields over the existing sources. The need for a better source of folic acid was obvious since only about 100 milligrams of the vitamin could be obtained from a ton of pig livers and about 300 milligrams from a ton of horse livers.

In the course of his work, Dr. Hutchings investigated a large number of molds as sources for the growth factor. Among these was the mold used to produce riboflavin. Assays were run on the filtrates from riboflavin tanks after the flavin had been removed. Fortuitously, the filtrate from one of these tanks contained considerable quantities of a growth factor when tested microbiologically, using *L. casei*, while filtrates from other batches of riboflavin did not exhibit any such growth stimulation. Immediately, a contaminant was suspected. A bacillus was isolated from the riboflavin tank and two bacilli were collected on agar plates exposed in the riboflavin plant. However, of the three possible contaminants only one of the bacilli produced much of the growth factor; this bacillus was isolated from the exposed agar plate. The growth factor produced by this bacillus was subsequently found to be different from the liver factor and was named the fermentation *L. casei* factor.[3]

The bacillus producing the fermentation *L. casei* factor was, of course, investigated extensively. It is an unidentified member of the genus *Corynebacterium*. Its nutritional and growth habits were determined, and soon production of research quantities of the factor was under way.

The investigation of the fermentation *L. casei* factor is now published history. Degradation[4] has shown it to be pteroyltriglutamic acid. The liver factor has been shown to be pteroylglutamic acid.[5] Pteroylglutamic acid has been synthesized[6] and placed on the market as a hemopoietic agent. The synthesis of pteroyltriglutamic acid by Dr. Boothe and co-workers will be presented in the following paper.

Soon after the isolation of the fermentation *L. casei* factor at Lederle, a request for some folic acid was received from Dr. Richard Lewisohn of Mount Sinai Hospital. Dr. Lewisohn and his co-workers had under way an extensive program of testing for regression of tumors in mice.

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Up to that time, the best results had been obtained with spleen and yeast extracts. The results suggested that some member of the vitamin B complex might be the responsible factor. The fermentation *L. casei* factor was supplied to this group of workers. In 1944, Leuchtenberger, Lewisohn, Lazlo, and Leuchtenberger[7] reported that the factor inhibited the growth of Sarcoma 180 transplanted in female Rockland mice. In a later publication,[8] it was shown that pteroylglutamic acid had no therapeutic effect on spontaneous breast tumors in mice, while the fermentation *L. casei* factor, pteroyltriglutamic acid, caused complete regression in 30 to 40 per cent of tumors in the animals. These findings have served as an impetus to the synthesis and to the clinical testing of pteroyltriglutamic acid. Some of the clinical findings will be presented in this series of papers by Dr. Klainer as well as by Dr. Lehv.

The antagonism between compounds promoting growth and those which inhibit growth has received ever increasing attention since Woods[9] described the interference of *p*-aminobenzoic acid on the action of sulfonamide. The research program at Calco and Lederle has involved the preparation of a number of analogues and homologues of pteroic acid. Some of these compounds possessed growth stimulation for the test organisms and animals, while others have actually inhibited the growth response of pteroylglutamic acid in the biological systems.

Recently, Hutchings and co-workers[10] reported the synthesis and inhibitory action of pteroylaspartic acid. This was the first purified and fully characterized derivative of pteroic acid to be described which had anti-pteroylglutamic acid action. More recently, Seeger and co-workers[11] reported the synthesis of a purified 4-amino- derivative of pteroylglutamic acid. The purifications of these antagonists are extremely important, in spite of the difficulty this entails. It is known that condensations, such as used in the preparation of pteroylglutamic acid, produce crude products which often have both growth promoting and inhibiting substances.

Dr. Smith will present the synthesis of a group of purified antagonists which have been tested routinely at Lederle. The pathological and experimental work on chicken sarcoma will be presented by Dr. Woll and Mr. Little. Dr. Meyer will present his clinical findings on the use of certain of these compounds in human leukemia.

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SYNTHESIS OF PTEROLY-γ-GLUTAMYL-γ-GLUTAMYLGLUTAMIC ACID (TEROPTERIN)

By J. H. BOOTHE, J. H. MOWAT, B. L. HUTCHINGS, R. B. ANGIER, C. W. WALLER, E. L. R. STOKSTAD, J. SEMB, A. L. GAZZOLA, AND Y SUBBAROW *

One of the simplest methods of synthesis of pteroylglutamic acid[1] involves the use of 2,4,5-triamino-6-hydroxypyrimidine, α, β-dibromopropionaldehyde and *p*-aminobenzoylglutamic acid. It was known that the fermentation *Lactobacillus casei* factor contained two molecules of glutamic acid[2] in addition to the one in pteroylglutamic acid, attached through peptide linkages. Therefore, the problem of synthesis of the fermentation *L. casei* factor was essentially to prepare a tetrapeptide consisting of one molecule of *p*-aminobenzoic acid and three molecules of glutamic acid. This could then, presumably, be used in the above reaction in place of the *p*-aminobenzoylglutamic acid.

Because glutamic acid has two carboxyl groups (known as the α or the γ carboxyl) there are two possible isomeric forms of a dipeptide of glutamic acid and five possible isomers of a tripeptide of 1(+)glutamic acid. This posed a problem as to which of the five isomers would correspond to the fermentation *L. casei* factor. The first indication as to which carboxyl group was involved in the peptide linkages was arrived at by synthesis of the two isomers containing two molecules of glutamic acid, *i.e.*, pteroyl-α-glutamylglutamic acid and pteroyl-γ-glutamylglutamic acid.

The former compound was synthesized by a general method first devised by Bergmann and Zervas.[3] 1(+)Glutamic acid was reacted with benzylchlorocarbonate to form carbobenzoxyglutamic acid, which was then treated with acetic anhydride to form carbobenzoxyglutamic anhydride. This anhydride was reacted with diethyl glutamate to yield diethyl carbobenzoxy-α-glutamylglutamate. The ester groups were removed by hydrolysis in dilute sodium hydroxide and the carbobenzoxy group was removed by hydrogenation in the presence of 10 per cent palladium-charcoal catalyst to yield α-glutamylglutamic acid. This dipeptide was then reacted with *p*-nitrobenzoyl chloride in a Schotten-Baumann type reaction to form *p*-nitrobenzoyl-α-glutamylglutamic acid. This nitro compound was reduced to the corresponding amino derivative by hydrogenating with platinum oxide and then reacted with 2,4,5-triamino-6-hydroxypyrimidine and α,β-dibromopropionaldehyde to form pteroyl-α-glutamylglutamic acid.

The γ-linked isomer was prepared by first reacting the carbobenzoxyglutamic anhydride already mentioned with ethanol at 120° C. to obtain α-ethyl carbobenzoxyglutamate.[4] This *mono* ester was treated with phosphorous pentachloride to form the acid chloride[5] and then with diethyl glutamate to yield triethyl carbobenzoxy-γ-glutamylglutamate. The carbobenzoxy group was reduced off as previously described and the triethyl γ-glutamylglutamate was isolated as the hydrochloride. This dipeptide was *p*-nitrobenzoylated in a Schotten-Baumann reaction which at the same time hydrolyzed the ester groups to yield *p*-nitrobenzoyl-γ-glutamylglutamic acid. The nitro group was converted to the amino group by hydrogenation using platinum oxide catalyst and was then condensed with 2,4,5-triamino-6-hydroxypyrimidine and α,β-dibromopropionaldehyde to yield pteroyl-γ-glutamylglutamic acid.

Comparison of the biological data of the two isomeric pteroyldiglutamic acids is shown as follows:

	S. faecalis R.	*L. casei*
Pteroyl-α-glutamylglutamic acid	0.5–1%	0.5–1%
Pteroyl-γ-glutamylglutamic acid	60–70%	60–70%
Pteroylglutamic acid	100%	100%

Since the α isomer showed a very low microbiological activity and the γ isomer a relatively high activity, it was thought that, probably, the three glutamic acids in the fermentation *L. casei* factor were connected through the γ carboxyl groups. Therefore, the γ-linked tripeptide was prepared through the following series of reactions.

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Triethyl-γ-glutamylglutamate, described above, was reacted with the acid chloride of α-ethyl carbobenzoxyglutamate to yield tetraethyl carbobenzoxy-γ-glutamyl-γ-glutamylglutamate. The carbobenzoxy group was removed by hydrogenation with palladium-charcoal catalyst and the resulting tripeptide treated with *p*-nitrobenzoyl chloride in benzene in the presence of diethyl amine. The resulting *p*-nitrobenzoyl tripeptide was then reduced to the corresponding *p*-amino derivative and condensed with α,β-dibromopropionaldehyde and 2,4,5-triamino-6-hydroxypyrimidine to form the pteroyl derivative. This pteroyl-γ-glutamyl-γ-glutamylglutamic acid showed the same microbiological activity as the fermentation *L. casei* factor:

	L. casei	*S. faecalis R.*
Synthetic compound	60%	2.0–6.0%
Natural compound	60%	2.0–6.0%

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A CASE REPORT OF METASTATIC CARCINOMA TREATED WITH TEROPTERIN*

By MAX J. KLAINER†

The clinical management of patients ith metastatic carcinoma has always presented difficult problems to the practicing physician. Not only have these cases invariably been fatal, but, in addition, the patient has all too frequently suffered great pain and has required considerable nursing care. It is, therefore, of great interest to report a case of metastatic carcinoma in whom not only the suffering was eliminated, but who has been restored to a life of utility by the use of a new chemotherapeutic approach.

Case Report. This patient, a 39-year-old Armenian housewife, was first seen in February, 1947, because of severe, agonizing pains originating in the region of the lower spine and radiating down the posterior aspect of the left thigh and left leg to the heel. A right radical mastectomy had been performed (by Dr. D. Richard Miller) at the Massachusetts General Hospital, Boston, in December, 1944, because of an adenocarcinoma. At the time of surgery, axillary nodes were found and removed. Pathological study reported grade III. The patient had made a good recovery from the operation and had done well until March, 1946, when pain developed in the region of the mid-lumbar spine. X-ray studies revealed a destructive process in the third lumbar vertebra. The patient then was given radiotherapy over a period of several months without any appreciable improvement. X-ray studies repeated in September, 1946, showed extension of the process in the third lumbar vertebra and new areas of destruction in the dorsal vertebrae and the ribs. A diagnosis of metastatic carcinoma was made and the patient placed on narcotic medication for relief of pain. However, she became progressively worse and soon was bed-ridden. She was unable to make the slightest movement without severe pain.

The patient's condition seemed hopeless and she was hospitalized primarily for the purpose of being kept as comfortable as possible. On admission to the New Eng-

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Teropterin is the name given by Lederle Laboratories Division, American Cyanamid Company, Pearl River, New York, for pteroyltriglutamic acid.

† Stoneham, Massachusetts.

land Sanitarium and Hospital, Stoneham, Massachusetts, the patient proved to be a trying nursing problem.* Only by lying quietly on her right side with her knees drawn to her abdomen could she endure her pain. Changing the bed linen, bathing, and the use of the bedpan were almost impossible. In addition, she sweated profusely, necessitating frequent changing of the bed linen. One hundred mg. demerol and dilaudid in doses of grains 1/16 every 4 hours only partially checked her pain.

Physical examination at the time of admission to the hospital revealed percussion tenderness along the lower dorsal and lumbar vertebrae and along the course of the left sciatic nerve. The eyes, ears, nose and throat, lungs, heart, and upper extremities were all within normal limits. The blood pressure was 140 mm. of mercury systolic and 90 diastolic, temperature 98.6, pulse 100, respiration 20 per minute.

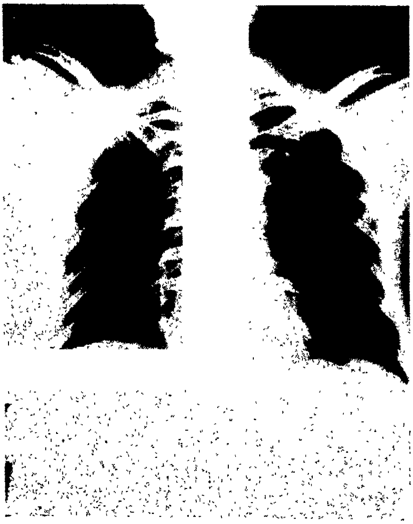

FIGURE 1.

Initial laboratory studies disclosed the following: hemoglobin, 12 g. (77 per cent) Sahli; red blood count, 3,660,000; white blood count, 5,450 with 40 per cent polymorphonuclears and 58 per cent lymphocytes. The total serum protein was 6.3 grams with 3.0 grams albumen and 3.3 grams globulin. No Bence Jones protein was found in the urine, and the urinalyses were not otherwise remarkable. In spite of extensive bone destruction, the acid phosphatase was essentially normal, 4.5 King units.

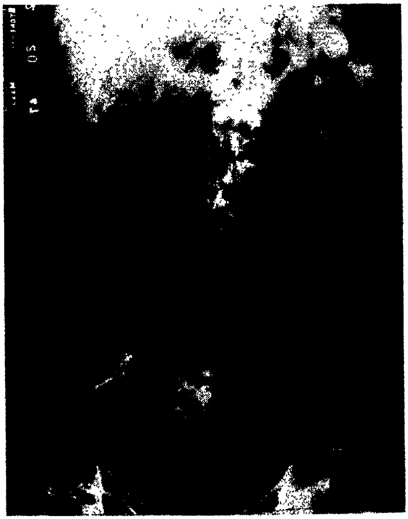

FIGURE 2.

X-ray films of the dorsal and lumbar spine, ribs, and pelvis showed innumerable areas of bone destruction with pathological fractures of several of the ribs and of the eighth dorsal and second and third lumbar vertebrae (FIGURES 1, 2, and 3). Areas of bone destruction were also found in the skull and in the right scapula. In an effort to check further bone destruction, the patient was placed on 150 mg. testosterone propionate given in 3 divided doses daily and 60 mg. teropterin, each being administered intramuscularly.†

During the first week of her hospital stay, the patient had her usual menstrual

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* The author wishes to thank the medical and nursing staff of the New England Sanitarium and Hospital, whose help in the management of the case was of great value.

† Teropterin had been submitted for clinical study by Dr. Y. SubbaRow, Director of Research, and his staff at Lederle Laboratories Division, American Cyanamid Company, to Dr. S. Farber of the Children's Hospital, Boston. I wish to express my thanks to Dr. J. Hawkins, formerly associated with Dr. Farber, for our original supply of the drug.

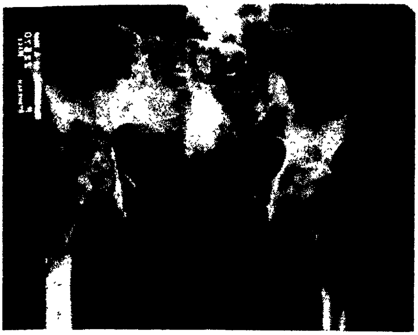

FIGURE 3.

period, and with the onset of the menses the pain increased markedly. An oophorectomy appeared advisable, and this was performed successfully on March 3, 1947, by Dr. Raymond Auvil of the hospital staff. At the time of operation, the uterus was found to be enlarged, but there were no masses in the liver or elsewhere in the abdomen. Pathological studies of the removed tissues revealed negative Fallopian tubes, but sections of the ovaries showed foci of metastatic adenocarcinoma.

In the first two weeks following surgery, the patient showed little change and continued to require large doses of narcotics for relief of pain. During this time, however, the testosterone propionate was reduced to 100 mg. daily while the teropterin was continued at 60 mg. daily. On March 17, 1947, the patient was given a transfusion of 500 cc. of citrated blood with prompt return of the hemoglobin and red count to normal levels.

The first evidence of improvement was noted on March 19, 1947. The pain became less marked, the sweats were markedly reduced, and the patient became more cheerful. About March 23, she suffered a slight setback with accentuation of pain, but this was attributed to her expected menstrual period. Immediately thereafter, progressive and marked improvement was noted. By April 1, 1947 practically all narcotic medication for relief of pain was eliminated and the testosterone medication completely discontinued. The

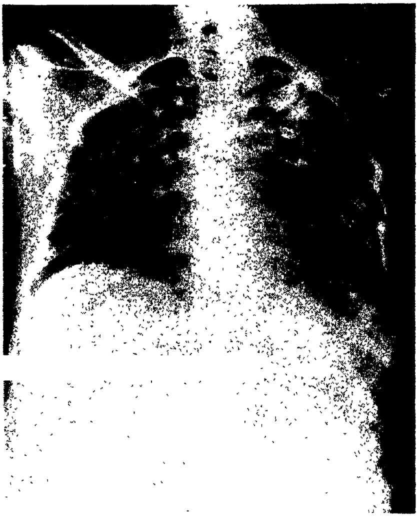

FIGURE 4.

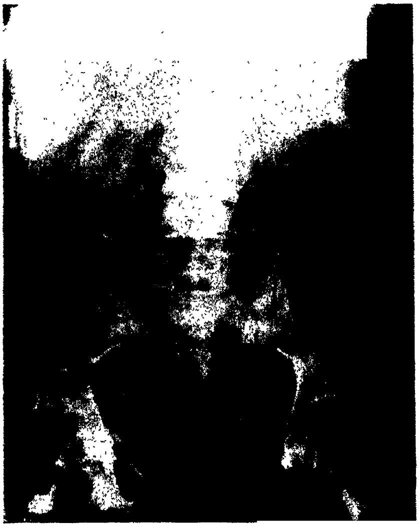

FIGURE 5.

patient now received teropterin intramuscularly, and barbiturates were given for sedation and aspirin compound tablets for control of pain. During the period of testosterone therapy, this patient had developed secondary sex changes such as

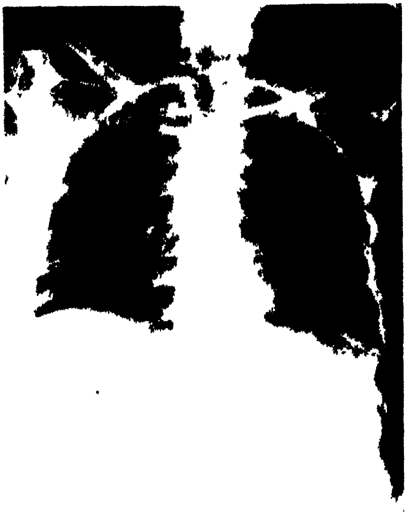

FIGURE 6.

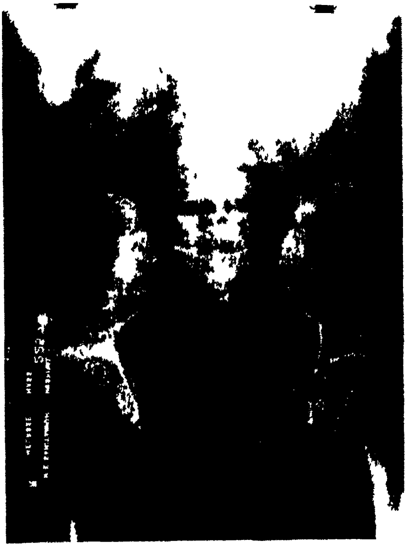

FIGURE 7.

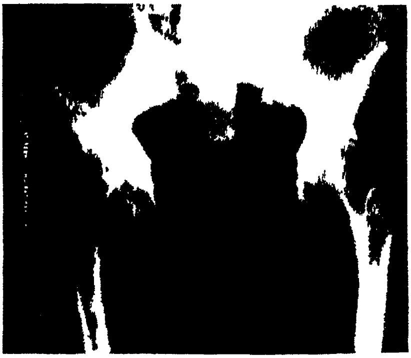

FIGURE 8.

hirsutism and deepening of the voice, which, however, slowly disappeared upon discontinuance of this drug. On April 11, this patient, who on admission had been unable to move without pain, dangled in bed. On April 14, she sat up in a wheel chair.

Repeat x-ray studies of the involved bones done on April 20 were interpreted as follows: "Reexamination of the skeletal system shows no significant increase in the areas of bone destruction. As a matter of fact several of the previously described areas of bone destruction appear smaller and there is new bone formation seen in several areas where the destruction was markedly pronounced. Several of the pathological fractures show evidence of bony regeneration. The compression fractures of the second and third lumbar vertebrae are still present as previously described but the bone structure is somewhat denser" (FIGURES 4 and 5).

In May, 1947, the patient was measured for a back brace. On May 14, she was allowed to walk, first with the aid of a walker and then with a cane only. This increase in activity produced no increase in the small amount of residual discomfort, and on June 5, 1947, she was discharged home on a maintenance dose of teropterin 20 mg. b.i.d.

X-rays taken at the time of discharge were interpreted as follows: "Films of various bones show a remarkable change in the appearance of the bone structure with disappearance of many of the previously described areas of bone destruction with recalcification of the bone and healing of many of the fractures, particularly of the ribs. The bone structure of the involved lumbar vertebrae has improved due to increased recalcification. The findings as a whole indicate a very marked improvement in the previously described findings of pathological fractures and metastases."

Since her discharge from the hospital, this patient has continued to improve both clinically and roentgenologically. Except for her back brace, she is now able to walk without the aid of any support and can perform many of her previous household duties. Except for transitory aching in the region of the left hip, which is readily relieved by an aspirin compound tablet, she is free of symptoms. The teropterin medication has been continued with a daily maintenance injection and she has learned to administer it herself. X-ray films that were taken in October, 1947, showed no new areas of bone destruction and further regeneration of bone formation in areas previously involved (FIGURES 6, 7, and 8).

The patient was last seen on December 1, 1947. At that time, she was completely ambulatory and was complaining only of mild aching in the lumbar spine on prolonged standing or walking.

Summary

In summary, a case has been presented of a 39-year-old woman with metastatic spread of an adenocarcinoma, presumably originating in the breast, in whom the combined use of oophorectomy, testosterone propionate injections, and teropterin injections produced definite clinical and roentgenological improvement. No claim has been made that teropterin alone produced the favorable results. However, the use of teropterin enabled the patient to discontinue the testosterone after the first month. Furthermore, it is interesting to note that the objective evidences of improvement, namely the x-ray changes, were not noted until several months after the oophorectomy and the discontinuance of testosterone therapy.

If the results obtained in this case were to be described in the manner in which surgeons commonly report their results in surgery on malignancies, it could be said that this patient with metastatic spread of a carcinoma to practically all her bones and to the soft tissues of the pelvis has, to date, obtained a 10-month "cure" by this combined chemotherapy and surgery.

USE OF TEROPTERIN IN NEOPLASTIC DISEASE: A PRELIMINARY CLINICAL REPORT

By S. P. LEHV, L. T. WRIGHT,
S. WEINTRAUB, AND I. ARONS[*]

A preliminary clinical report is presented here on the use of teropterin in a series of cases of malignancy in Harlem Hospital. Since it is a preliminary report, we are confining ourselves to the presentation of our findings without attempting to interpret them or draw premature conclusions.

The study was started in August, 1947, by a group composed of Drs. Wright, Weintraub, Arons, and the writer. We confined our selection to those cases which were considered hopeless and terminal by virtue of extensive carcinomatosis beyond the reach of any of the other approved methods of therapy. In fact, our early cases were moribund. We felt that any favorable response in such cases would justify further study.

Accordingly, fortified by the original animal work of Lewisohn, Leuchtenberger, *et al.* [1, 2, 3] as well as the clinical observations of Dr. Farber's group[4] in Boston, teropterin therapy was instituted as a sole form of treatment. This was done to provide as controlled a study as possible.

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With the exception of two cases where surgery and stilbestrol were used, respectively, the patients in our group were given no other form of therapy, excepting symptomatic routine medication. It must be remembered that we were dealing with adults, mostly middle-aged, and we met with the complications of intercurrent infections and the usual degenerative diseases.

Teropterin (Lederle) is a yellow powder. It is instantly soluble in normal saline, is apparently stable at room temperature, and is completely painless on injection. No toxic effects were noted in animals, none in the series of over one hundred cases in humans treated by the Boston group, and none in our series. The chemistry and pharmacology of teropterin need not be repeated here.

With the assurance given us by the above data, we did not hesitate to undertake this clinical study. Of thirty-three treated cases, we are reporting twenty which to this date have been observed for periods of from one to three months. The remaining cases have not been under our observation long enough to be included.

TABLE 1

TYPE AND LOCATION OF TUMOR

Carcinoma of prostate	4
Malignant lymphoma	1
Carcinoma of digestive tract	10
Malignant melanoma	3
Osteogenic sarcoma	1
Metastatic carcinoma of neck	1

METHOD OF STUDY. In addition to the customary work-up, the patients were observed for subjective and objective changes. The following studies were made in each case: x-ray studies, multiple biopsies, chemistry, and hematological values. The x-ray studies were made at approximately two-week intervals, the hematology was done weekly, and the chemistry weekly when abnormalities were found. Observation of the patient's activity was carefully noted together with the patient's own opinion of his progress.

The histological findings and x-ray studies will be given in a later report.

The following laboratory studies were made in each case:

(1) Urine analyses.
(2) Hematology: in the form of red and white blood counts and hemoglobin
(3) Liver function tests:
    a. VandenBergh.
    b. Thymol turbidity
    c. Bromsulfalein retention
(4) Blood: creatinine, urea nitrogen, and glucose.
(5) Acid and alkaline phosphatase
(6) Total protein and A/G ratios
(7) Kahn test

In this group of neoplasms, most of them far advanced, there was a conspicuous absence of markedly abnormal chemistry values. In a few of our cases where the liver was grossly involved with enormous palpable enlargement, the liver function tests were normal.

FINDINGS: *Urine Analyses.* No changes were noted during treatment.

Hematology. The red blood cell count and the hemoglobin values were watched closely because of the relationship between teropterin and folic acid. Occasional abnormalities in the white blood cell count were noted, which appeared as a diminution in the lymphocytes, both absolute and relative. Despite changes in the chemistry and clinical improvement in some patients, the red blood cell count and hemoglobin values were not appreciably affected. In some cases, supportive therapy was needed in the form of transfusions of whole blood and plasma.

Liver Function Tests. Of the twenty cases, only five were found that showed abnormal liver function. Of these, two showed improved excretion of bromsulfalein; one went from a 45 per cent retention to 30 per cent and the other from a 15 per cent retention to 0. Both died.

Alkaline Phosphatase. In our series, abnormalities were noted in eleven cases. Two cases showed values just above normal limits, a third showed no change after one month's treatment, the fourth increased after two months' treatment, and the fifth dropped from 17.7 Bodansky units to 6.1, but then became elevated to 17.7 just before death. The remainder of the cases showed a decrease. In compilation, therefore, nine cases showed no abnormalities, and, of the remaining eleven, six improved and five are more or less indeterminate.

Acid Phosphatase. These data are considered insufficient, but, for the purpose of record, are reported. In four cases of prostatic cancer, we noted in one (W.D.) a temporary rise from 10.6 to 21.8 and then a fall to 0.25 after one month's treatment. The alkaline phosphatase in this case fell from 22.2 to 16.9 in the same length of time. The second case (E.G.) showed a drop from 16.8 to 8.2 in the acid phosphatase, while the alkaline phosphatase remained at a high normal level. The third (D.K.) demonstrated a relatively normal acid phosphatase reading—6.03—but a high alkaline phosphatase of 40.0. Within five weeks after treatment was instituted, the alkaline phosphatase in this case came down to 24.8 and the acid phosphatase to 1.7. In this same patient,

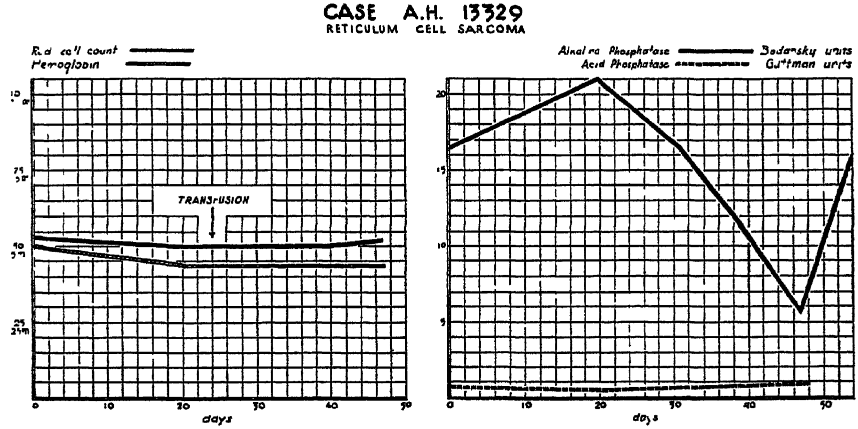
CASE A.H. 13329
RETICULUM CELL SARCOMA

FIGURE 1.

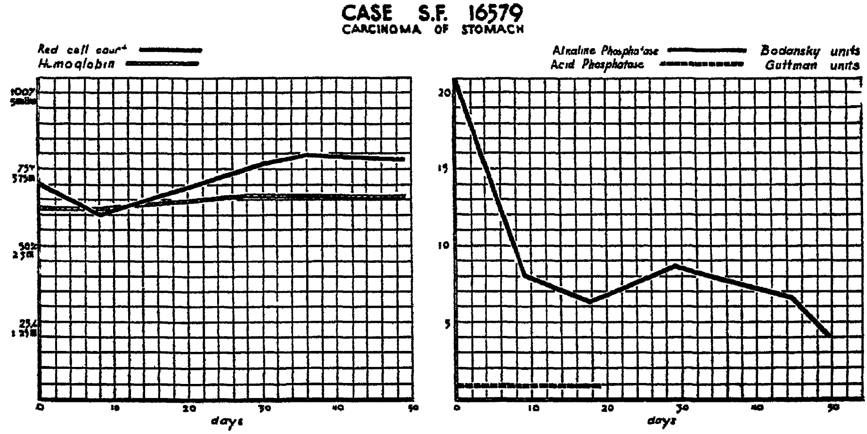
CASE S.F. 16579
CARCINOMA OF STOMACH

FIGURE 2.

it was noted that liver function tests were normal; the bromsulfalein retention was 0, the thymol turbidity stayed within the range of 2-3, and the VandenBergh gave a negative direct reaction. The fourth case showed no change in the acid phosphatase, which remained at a low level, but did show a drop in the alkaline phosphatase from 15.3 to 4.0 after six weeks of treatment.

The other blood and chemistry values were taken to help us in clinical management and evaluation.

Figures 1 and 2, taken from the cases of patients A.H. and S.F., are illustrative of our findings in two instances.

Discussion. It would appear that, by virtue of the lack of consistently abnormal chemical and hematological findings, we cannot depend upon those values alone for prognosis and should consider them with caution as a diagnostic aid. We do feel that, in a few cases, they have given us a lead as to general progress, and the fall in alkaline and acid phosphatase has paralleled general clinical improvement.

It is true that, in those cases where no improvement was noted in the chemical values, the patients died. Some of the cases showed improvement in chemistry but died nevertheless. Furthermore, many cases showed no abnormal values at all from start to finish.

We have been impressed with the absence of hematinic response in those patients who received treatment. In a number of cases, we found a moderate rise in the red blood cell count and hemoglobin values, but nothing startling. Despite an apparent improvement in the chemistry values and liver function, as well as the assurance of these very patients that they never felt better, it was necessary to transfuse them.

The chemistry findings served only as corroboratory findings in the management and interpretation of results with the use of teropterin.

The predominating symptoms in our series were pain associated with malaise and local symptomatology referable to the disturbed physiology of the part affected. There was the usual history of loss of weight and strength, but for the most part the patients applied for hospitalization because of pain and inability to maintain certain body functions.

Drug Administration. After a preliminary work-up, consisting of x-ray studies, blood chemistry, and hematology, the patients at the start were placed on teropterin 50 mg. daily, single, intramuscular dose, on the recommendation of Drs. SubbaRow and Farber.[5] At present, we are using 10 mg. twice daily. In one case, where a 10 mg. dose was inadvertently omitted, the patient complained of distress, and when this omission was corrected his complaint disappeared. We have noted no significant difference when teropterin was given in single or divided doses. When a patient missed medication for one or two days, we found an accentuated response to the drug upon re-institution of treatment. We are now making further studies along this line.

It was found that there was no particular advantage to be gained by intravenous administration which would outweigh the disadvantages of such a route.

As mentioned above, none of the patients was given any other form of cancer therapy. The patients were not informed that they were receiving anything special or new. This was considered important, since we wished to eliminate or at least minimize the psychogenic factor. To them, it was another among many "needles," and we can safely deny the psychogenic element at the start of the study. It was not long, however, perhaps because of the regularity of the injections or the response to them, that the patients became aware of the therapy. In cases where treatment lasted many weeks, we recognize the possibility of superimposition of psychogenic enthusiasm.

What, then, has been the response to this therapy? Almost without exception—and this includes patients who were practically moribund—the reaction has been a feeling of well-being characterized by most in the expression, "I feel like a new person." There has been a sharpening of the sensorium with a new-found interest in surroundings, and a desire for ambulation as compared to a previous state of apathy and listlessness. One patient, when asked how he felt, replied that he felt so improved that he expected to go home shortly. He died nine hours later. This feeling of being well usually became evident within a few weeks after treatment had begun, but, in some cases, strangely enough, the favorable reaction came on as early as after the third injection. We have all seen the post-transfusion response in patients where they admit to a feeling of well-being and contentment, but the response after teropterin therapy transcends that feeling and can, perhaps, best be described as "cheerfulness."

Along with this tonic response, there came, in many cases, a reduction in the amount of opiates needed for control of pain, as noted in TABLE 2. Abstracts from a few case histories may bring this more sharply into focus:

H.A., age 47, was admitted July 18, 1947. He had a reticulum cell sarcoma. We started treatment on him on August 18, 1947, and he became pain-free in ten days. He was able to get out of bed seventeen days after the institution of treatment and remained pain-free for fifty-one days until his death on October 17, 1947.

W.C., age 61, was admitted July 14, 1947. He had a malignant melanoma of the rectum. We started treatment on him on August 14, 1947, and he became pain-free in seventeen days.

E.H., age 46, was admitted September 5, 1947. She had a carcinoma of the colon with metastases with complaints of abdominal pain, anorexia, and dyspnea. We started treatment on September 22, 1947, and thirteen days later the patient stated that she had been able to have her first good meal in a month. She was happier, more communicative, and ambulant. There was no need for sedation after

TABLE 2

SHOWING REDUCTION IN THE AMOUNT OF NARCOTICS USED OR NEEDED IN A SAMPLING OF CASES

Patient	Before therapy			After therapy		
M.S.	Demerol	200 mg.	31 days	Demerol	0	107 days
	Morphine sulfate	1.5 g.		Morphine sulfate	0	
	Codeine	10 g.		Codeine	3 g.	
E.H.	Demerol	975 mg.	25 days	Demerol	100 mg.	44 days
	Morphine sulfate	0		Morphine sulfate	0	
	Codeine	2 g.		Codeine	0	
L.J.	Demerol	300 mg.	4 days	Demerol	250 mg.	64 days
	Morphine sulfate	0		Morphine sulfate	0	
	Codeine	½ g.		Codeine	0	
S.F.	Demerol	200 mg.	17 days	Demerol	0	69 days
	Morphine sulfate	½ g.		Morphine sulfate	0	
	Codeine	1 g.		Codeine	0	
A.H.	Demerol	2400 mg.	30 days	Demerol	1700 mg.	60 days
	Morphine sulfate	0		Morphine sulfate	0	
	Codeine	0		Codeine	0	

October 18, that is, twenty-six days after the institution of treatment. She took a sudden change for the worse on November 3 and died on November 6, 1947.

C.B., age 47, was admitted on August 14, 1947. He had a carcinoma of the prostate and complained of pain in his right leg and sacrum and burning on urination. We started treatment on August 27, and he became pain-free almost immediately.

R.P., age 46, was admitted on August 15, 1947. He had a carcinoma of the stomach with extensive metastases. We started treatment on October 27, 1947, and he became pain-free after the second dose, stating that he had had his most comfortable night in weeks. His condition, however, remained poor and he died on November 26, 1947.

In this series, there were five deaths; all died pain-free without need for narcotics. They remained clear in mind until death came. In two or three cases, we noticed spiking of the temperature curve prior to exitus. The low grade spiking as a possible expression of lysis with absorption is under study at present. One incidental finding was a flattening of the pulse curve after a week or two of treatment.

In some cases, there was a rather dramatic response to the therapy. The following two cases are illustrative:

B.H., aged 65, presented an inoperable carcinoma of the esophagus. He had been treated elsewhere by radiation therapy and the implantation of radon seeds, which was followed by stricture of the esophagus, necessitating repeated dilatation. He had been unable to swallow anything but liquids for a number of months. Teropterin therapy, 50 mg. by single intramuscular injections, was given daily. After the third injection, the patient began to eat oatmeal and small meat portions. After the fifth injection, he ate a chicken dinner. Within ten days after treatment had been instituted, it was noted that his blood pressure dropped from 220/100 to 196/90, and the patient had gained six pounds. There was no appreciable change in the following two weeks, but at that time the patient reported an episode of profuse blood-tinged expectoration. The chest was examined and found to be negative. At this time, also, he described a tightening in his throat and was unable to eat solids for a period of one week. In the fourth week of treatment, the patient again improved and was able to eat solid food satisfactorily. Fluoroscopy at that time showed a change from the original string-like passage of barium to a pencil-like passage. After five weeks, the patient's blood pressure was 174/88. At present, he is maintaining his weight and working eight hours daily.

E.G., aged 65, was admitted on October 25, 1946, with inoperable carcinoma of the prostate. He had extensive metastatic lesions of the skeleton with spinal cord involvement. Clinically, he presented evidence of almost complete paresis of the lower extremities with anesthesia of the lower right extremity and partial anesthesia of the lower left extremity. There were lancinating pains associated with spasticity of the lower limbs. The legs were edematous and showed secondary joint changes. In December of 1946, an orchidectomy was performed. In early 1947, the patient received radiation therapy for a period of about three months. His response to this form of therapy was only moderate, and, because of the continued spasticity of his legs, he was given a short course of curare. Following this, physiotherapy was administered for two months. From the date of admission until teropterin therapy was instituted on October 13, 1947, the patient was largely bed-ridden, despite orchidectomy and all other forms of therapy. Three weeks after teropterin was started, he felt sensation returning to his legs and an increased ability to move them. At present, he is able to spend most of his time in a wheel chair and in a walker. One week ago, he walked un-supported for a distance of fifteen feet. He is improving daily, and is completely pain-free. A neurological examination was made on December 2, 1947. The examiner found, "Patient is devoid of anxiety and is hopefully looking forward to his eventual recovery. The comparative examination is as follows: On October 26, 1946, he had spasms of thigh muscles, more marked on the left. Today, none is present. The marked paresis of the lower extremities has been replaced by good function. The patient is able to flex, extend, and to bicycle ride while lying on his back. He walks with a somewhat spastic gait while holding on to the bed. He is a little unsteady when walking without support, but much is due to fear and to long disuse of the balancing mechanisms. The sensory picture has returned to normal, there being no dysaesthesias and no sensory level. Reflexes of the knees are hyperactive and the ankle jerks are a little hypoactive.

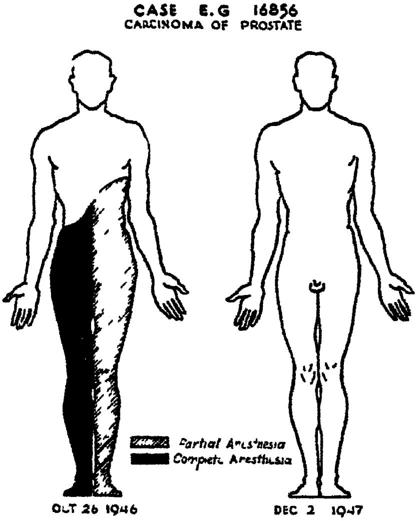

FIGURE 3.

Opinion. This patient's improvement is phenomenal, and he should be able to walk up and down stairs within the next three weeks. Nutrition has markedly improved—any further weight gain would put him into the class of early obese." (See FIGURE 3.)

Summary

Twenty of thirty-three teropterin-treated cases of malignancy are reported. These patients have been under exclusive teropterin therapy for a period of one to three months.

Laboratory studies showed that alkaline and acid phosphatase values, when abnormally high, showed a tendency to decrease following teropterin therapy. The red blood cell count and hemoglobin values were essentially unaffected.

Our clinical observations indicate that the drug is non-toxic in the dosage given. Pain was relieved in most cases, if not all, by the use of teropterin. This obviated, in the main, the necessity for further use of opiates with its concomitant depressive effects. All patients exhibited a sense of well-being, cheerfulness, and a state of mental alertness previously unseen by us in advanced malignancy. Fifteen of these twenty cases are still alive and under treatment.

Although this group of cases is too small for definite conclusions, it is our impression that teropterin therapy is a valuable adjunct in the management of malignancy, and further clinical investigation is warranted.

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THE CHEMISTRY OF CERTAIN PTEROYLGLUTAMIC ACID ANTAGONISTS

By JAMES M. SMITH, JR., DONNA B. COSULICH, MARTIN E. HULTQUIST, AND DORIS R. SEEGER[*]

Chemical analogues which are metabolite antagonists are known for a large number of essential metabolites, including all the known members of the vitamin B complex. Analogues of pteroylglutamic acid (I) have been studied extensively by the Research Groups at the Lederle Laboratories Division and the Calco Chemical Division, American Cyanamid Company.

The analogues to be discussed in this paper are of three main types: First, the analogue in which the glutamic acid is replaced by another amino acid, the remainder of the molecule being unchanged; second, an analogue in which the substituents on the pteridine ring are different; third, an analogue in which a substituent is introduced on the amino group of the *p*-aminobenzoylglutamic acid part of the molecule; and, finally, an example of a combination of two variations. All of these analogues have been isolated in pure form.

The method of synthesis is the Waller reaction[1] in which 2,4,5-triamino-6-hydroxypyrimidine, 2,3-dibromopropionaldehyde, and *p*-aminobenzoylglutamic acid are mixed in water at pH 3-4.

This is a general reaction for the synthesis of pteroylglutamic acid analogues. Other *p*-aminobenzoylamino acids may be substituted for *p*-aminobenzoylglutamic acid, as, for example, *p*-aminobenzoylaspartic acid. Other 4,5-diaminopyrimidines may be substituted for 2,4,5-triamino-6-hydroxypyrimidine, as, for example, 2,4,5,6-tetraminopyrimidine. For *p*-aminobenzoylglutamic acid, *p*-(N-methylamino)benzoylglutamic acid may be substituted, which gives a methylpteroylglutamic acid.

Pteroylaspartic acid (II) was prepared by the above method soon after the structure of pteroylglutamic acid had been elucidated, and the preparation and properties of this compound have been described in detail by Hutchings *et al.*[2] It is an antagonist for pteroylglutamic acid in the growth of *Streptococcus faecalis* R.

The analogue having an amino group instead of the hydroxyl in the 4-position, N-[4-{[(2,4-diamino-6-pteridyl)-methyl]amino}benzoyl]glutamic acid (III), has been reported by Seeger, Smith, and Hultquist.[3] It was synthesized by the Waller reaction as described above and purified by a process very similar to that for pteroylglutamic acid.[1] It was isolated both as the free acid and as the magnesium salt. The 4-amino derivative is considerably more soluble than pteroylglutamic acid. The structure was assigned by comparison of the ultraviolet absorption data with that for pteroylglutamic acid and by degradation studies similar to those used for structure proof of the latter. There is little doubt that the structure is as given, with the side chain in the 6- position on the pteridine ring and amino groups in the 2- and 4- positions. The details of this work will be published elsewhere.

The 4-amino derivatives are powerful antagonists for pteroylglutamic acid in the growth of *Streptococcus faecalis* R. A number of 2,4-diamino-6,7- disubstituted pteridines which also have antagonist activity have been reported by Cain, Mallette, and Taylor.[4]

A methyl group was introduced into the pteroylglutamic acid molecule by using *p*-(N-methylamino)benzoylglutamic acid in place of *p*-aminobenzoylglutamic acid in the reaction described before.

The product, N-[4-{N-[(2-amino-4-hydroxy-6-pteridyl)-methyl]-N-methylamino}benzoyl]glutamic acid (IV),[5] was purified by a method similar to that described previously. The pteroic acid analogue, 4-{N-[(2-amino-4-hydroxy-6-pteridyl)methyl]-N-methylamino}benzoic acid (V),[5] was also prepared in a similar reaction using *p*-(N-methylamino) benzoic acid.

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The structures are assigned from ultraviolet absorption data and degradation studies. These analogues are antagonists.

A recent publication, by Wolf *et al.*,[6] describes the structure of rhizopterin, formerly known as the "S.L.R. factor," as *p*-[N-(2-amino-4-hydroxypyrimido[4,5-b] pyrazin-6-ylmethyl)-formamido]-benzoic acid. It is interesting that this compound, which has a formyl group in the same position as the methyl group in our compound (V), is a growth factor rather than an antagonist for the growth of *Streptococcus faecalis* R.

Examples of a combination of these variations in the structure of pteroylglutamic acid are N-[4-{N-[(2,4-diamino-6-pteridyl) methyl]-N-methylamino} benzoyl] glutamic acid (VI) and 4-{N-[(2,4-diamino-6-pteridyl) methyl]-N-methylamino} benzoic acid (VII). This type has an amino group in the 4- position as well as a methyl group on the *p*-amino nitrogen of *p*-aminobenzoylglutamic acid.

A number of other variations and combinations thereof have been made in the pteroylglutamic acid molecule. A detailed discussion of these compounds is beyond the scope of this paper, but reports will appear in the literature subsequently.

The biological activity of these compounds has been examined by Drs. B. L. Hutchings and E. L. R. Stokstad of the Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y. A comparison of the antagonist activity of a series of analogues is given in TABLE 1.

TABLE 1

RELATIVE ACTIVITY OF PTEROYLGLUTAMIC ACID ANTAGONISTS (PURIFIED)

Compound No.	Antagonist activity (*Streptococcus faecalis* R)
III	700
IV	100
V	15
VI	800
VII	162

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THE ROUS CHICKEN SARCOMA IN BIRDS TREATED WITH FOLIC ACID AND ITS DERIVATIVES: A PATHOLOGICAL STUDY

By EPHRAIM WOLL*

The purpose of the present study was (1) to evaluate the natural history of the tumor in the untreated bird; (2) to define the natural variations of its course, gross and microscopic features; and (3) to compare these with the findings in birds treated with folic acid, its derivatives, and in birds maintained on a folic acid deficient diet.

REVIEW OF LITERATURE

The subject of filterable tumors in fowl was reviewed by Gye and Purdy in 1931,[1] and by Foulds in 1934.[2] The preceding studies were concerned mainly with the problems of etiology, transmission, and immunology. Morphology of the tumor was studied among others by Rous,[3, 4, 5] Peyron,[6] Roskin,[7] Fujinami,[8] and Foulds.[9]

From the time of its discovery in 1910, when transmission was at best difficult, and possible only to birds actually related to the original animal, successive tumor generations have now reached a state of great virulence. The tumor is now readily passed to other breeds of chickens. In our hands, it has been 84.4 per cent successful in 5-7 week-old chickens and 100 per cent successful in baby chicks. The latter finding is in accord with that of Sugiura.[10]

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The original tumor, as described by Rous,[3] was a well-defined, firm, opaque mass which shelled out readily. In the center coagulation, necrosis was marked. The rim was composed of friable, yellowish-pink, finely striated tissue. The typical microscopical pattern was that of loose bundles of spindle cells with vesicular nuclei and intercellular fibrils. Small round cells with a myxomatous intercellular substance were also seen at the advancing edges of the growth. The actively growing tumor is now much more infiltrative. especially in the baby chick. Hemorrhage, infarction, liquefaction, and myxomatous degeneration are prominent features. The typical cell is more likely to be small, round, growing in irregular masses, with scant stroma.

Careful studies were made, by Rous and Murphy, of the histology of the tumor in resistant animals.[4] The characteristic findings were: (1) Large local and diffuse collections of lymphocytes. These were first seen near the vessels and periphery of the tumor. (2) Attenuation of the spindle shape. (3) Hydropic degeneration of the cytoplasm. (4) Presence of multinucleated giant cells. (5) Profuse ingrowth of capillaries and fibroblasts. (6) Ultimate disappearance of the tumor with or without residual scarring. Rous and Murphy found that in the first 2 days after grafting the tumor cells proliferate actively in the susceptible as well as the resistant host. The subsequent days are marked by either continued growth or retrogression.

MATERIALS AND METHODS

The conclusions of this paper are based on gross and microscopic examination of approximately 350 birds ranging in age from 2 days to 10 weeks. The source of the inoculum, method and site of introduction, and other related information are described by Little *et al.* in the following paper.[11] The breeds included New Hampshire Red and a small number of Plymouth Rock. In almost all instances, the animals were killed with chloroform or other suitable methods, and autopsied within one hour of death. Representative blocks of the tumor were fixed in Zenker-formol solution. A large number were fixed in neutral formaldehyde and Zenker-acetic acid. The sections were stained with hematoxylin and eosin, and a small number with eosin-methylene blue, phloxine-methylene blue, and Shorr's modification of Masson's trichrome stain. A few sections were fixed and stained for glycogen and mucin. In all cases, the viscera were inspected and all gross evidence of metastasis or any other departure from the normal was noted. While numerous blocks of tissue from the viscera were taken, no exhaustive study of the normal or abnormal findings in the treated or control animals was made. This phase of the problem is being investigated now.

The gross and microscopic findings were tabulated in terms of criteria to be described below. In the interest of objectivity, many of the gross and all of the microscopic examinations were made without previous knowledge of the treatment used.

RESULTS AND ANALYSIS

Pathology of the Untreated Birds—*General Considerations.* Most of the past study of this tumor was expressed in static terms. It was described as it appeared at any given time, without consideration of the antecedents and consequents. In the original studies of Rous and Murphy, the tumor was described dynamically, in its natural history as well as in relation to the host.[4, 5] Just as there are slowly and rapidly growing variants of the same tumor, there are corresponding morphological differences between the two. Surely these differences are not absolute. Individual elements characteristic of relative benignancy or malignancy are seen in a given tumor, but the overall summation permits judgment at least of the two extremes of this range.

TABLE 1

GROSS FINDINGS

	Rapid	Slow
Skin	Discolored, blue-green	Clear
Subcutaneous space	Edematous, massive hemorrhage, occasional tumor extensions	Clear
Tumor consistency	Soft, occasionally fluctuant	Firm to hard, occasionally nodular
Size	Large, limited by anatomical boundaries	Small, occasionally minimal, discrete
Margins	Vague	Sharp
Cross section		
Color	Light grey, translucent, mottled	Pale yellow, opaque, homogeneous
Rim	Translucent, occasionally opaque	Opaque
Center	Liquefied, viscid, hemorrhagic, occasionally cystic	Firm, homogeneous, occasionally small central areas of myxomatous liquefaction
Lobulations	Absent	Often present
Hemorrhage	Severe, early, massive	Slight to moderate early
Infarction	Massive	Absent or minimal
Myxomatous change	Marked or absent	Absent

TABLE 2

MICROSCOPIC FINDINGS

	Rapid	Slow
Type cell	Small, round or spindle, in fascicles, occasionally irregular masses	Giant round, or fibroblast in thick fascicles
Cytoplasm	Light purple, homogeneous	Pink, granular, vacuolated
Mitoses	2-3, occasionally 4-5	Rare
Vascularity	Sinusoidal	"Granulomatous"
Stroma	Scant	Abundant
Lymphocytes	Few	Numerous
Polymorphs	Very numerous	Numerous
Macrophages	Few	Many
Giant cells	Absent or rare	Numerous
Fibroblasts	Rare	Most common cell
Collagen	Very scant	Scant
Invasion	Rapid, active	Slow or absent
Metastases	Common	Extremely rare

Criteria. TABLES 1 and 2 summarize the gross and microscopic differences between the slowly and rapidly growing tumors.

External examination. The earliest naked-eye evidence of tumor growth is noted 3 days after intramuscular inoculation. In 10 to 20 days, a small nodule can be felt in the muscle at the site of injection. In the untreated animal. the tumor grows rapidly and steadily until the whole side of the thorax bulges forward yet retains a normal contour. Maximal growth is reached in the next 7-10 days, at which point the animal usually dies. The largest growth is, apparently, only limited by the anatomical boundaries of the fascia, muscle bundles, etc. In no case was the tumor seen to extend across the mid-line. On palpation, the tumor is often fluctuant. The overlying skin is freely mobile, markedly discolored a deep blue or green. Occasionally, a separate discrete tumor mass is felt in the subcutaneous space or corium.

This is apparently a growth related to the needle track of the inoculation

Subcutaneous Space The blue discoloration is apparently related to the hemorrhage It is to be noted that even in the most severe old hemorrhage the amount of hemosiderin is small.

Necrosis The amount of liquefaction, hemorrhage and necrosis may be particularly large so that the tumor is transformed into a cystic cavity lined with a relatively thin layer of viable tissue. It is of interest to note that small nodular masses of young tumor cells. as judged by the above criteria. can be seen growing on the inner lining of such cavities. It would seem. therefore, that the liquefied material, if not favorable, is at least not an inhibitory *milieu* for growth. Something akin to this is seen in the presence of intra-abdominal masses of tumor where viable, actively growing cells are seen in the midst of mucinous and hemorrhagic fluid. Rous[5] comments on the "culture" appearance of tumor growing on the surface of the liver without any vascular connection with the latter. Similar growths were observed in chick embryos.

Lobulations. These, on microscopic examination, are bands of connective tissue with accompanying capillaries entering the tumor from the adjacent musculature.

Hemorrhage. This is most prominent in the full-blown type, yet hemorrhage is a frequent accompaniment of early rapid growth. It was seen 3 days after inoculation. Several observers speak of a hemorrhagic disease in chicks in association with the sarcoma. In our experience, hemorrhage was invariably associated with tumor growth and not evident independently.

Mucin. It is not clear whether this substance, staining purple with hematoxylin and deep blue with Myer's muchematein stain. is a product of degeneration due to ischemia or a substance elaborated by viable, actively proliferating, undifferentiated cells It is seen in both cases, more commonly in the former.

Type Cell. The least differentiated variant is a small round cell with a vesicular nucleus, prominent compact nucleolus, scant, very finely granular purple cytoplasm, and poorly defined cell margin. These cells lack any specific configuration. At times, they line the sarcolemma lining of a space formerly occupied by a muscle fiber. A more differentiated variant is a small spindle cell with essentially similar features as above plus delicate cytoplasmic processes in the long axis, resembling the mesenchymal fibroblasts. The retrogressing tumor shows numerous swollen spindle or round cells with a clear peripheral zone of cytoplasm. Occasionally, these are multinucleated. Careful search for intracellular fibrils does not show these to be present in any of the tumor cells. nor is there any stainable glycogen.

Mitosis. This is not a prominent feature of the tumor. The frequency is from an occasional mitotic figure in many fields in the slow tumor to 4 or 5 per high-power field in the rapidly growing type. Mitotic figures are particularly common in the tumors of baby chicks. They are apparently normal. In giant tumor cells, the chromosomes occasionally appear fragmented and are arranged radially. In only one instance was a triaster seen; this isolated observation is therefore unreliable. Grading of the tumor on the basis of mitotic activity is useful only at the two extremes, namely, the slow and rapidly growing tumors

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Captions for FIGURES 1-6 (*see opposite page*).

FIGURE 1 Tumor 3 days after inoculation Note the gray translucent nodules with surrounding hyperemia (untreated)

FIGURE 2 External view of tumor inoculated in the neck (untreated).

FIGURE 3. Slowly growing tumor in naturally resistant 5-week-old chicken. Note the light yellow, opaque, confluent, nodular masses (untreated).

FIGURE 4. Cross-section view of tumor in baby chick treated with folic acid-free diet and folic acid supplement.

FIGURE 5. External view of tumor in a bird treated with 4-amino-folic/d(-) glutamic acid. Note the small size and discrete character of growth.

FIGURE 6. Cross section view of tumor in FIGURE 5.

1

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3

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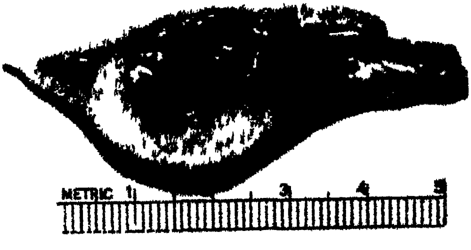

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FIGURE 7. Early growth. Note the displacement and compression of the muscle fibers. Shorr's modification of Masson's trichrome stain, X300.

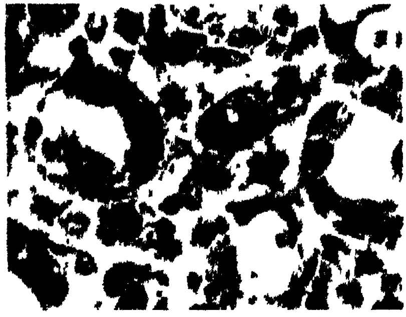

FIGURE 8. A somewhat later growth than in FIGURE 7. Note the arrangement of the tumor cells along the sarcolemma and the myxomatous change Hematoxylin-eosin X300.

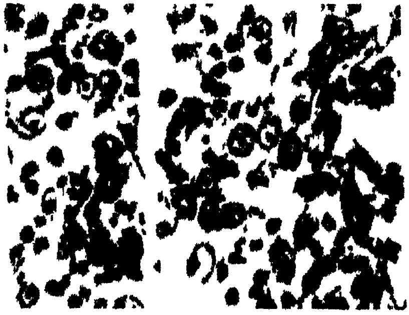

FIGURE 9. Undifferentiated cells of a rapidly growing tumor. One degenerating cell is also shown. Hematoxylin-eosin, X270.

FIGURE 10. Spindle cells of a rapidly growing tumor Shorr's modification of Masson's trichrome stain, X300.

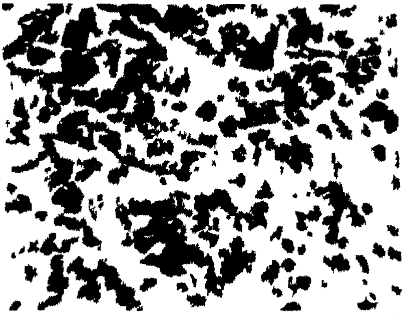

FIGURE 11. Tumor in a naturally resistant animal Note the abundance of fibroblasts, lymphocytes, and collagen. Hematoxylin-eosin, X270.

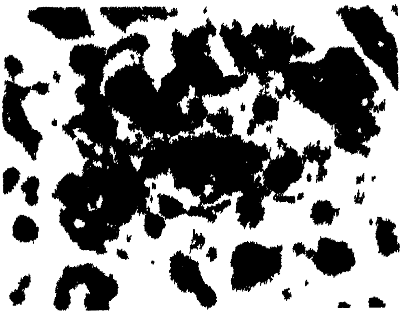

FIGURE 12. Hydropic degeneration and swelling of tumor cells. Note the vacuoles in the nucleus and cytoplasm.

Stroma. This forms the most prominent feature of the slowly growing variant. It is difficult to decide whether the fibroblasts are differentiated tumor cells, proliferations from adjacent normal tissue, or a mixture of both. The association of capillaries and collagen inclines one to the last two possibilities.

Invasion. In general, the tumor is of low invasiveness except in the baby chick.

It usually insinuates itself between muscle fibers or other anatomical barriers. In a rapid advance, a good deal of degenerating muscle is seen in the depths of the tumor. On gross examination, the opaque yellow rim of an otherwise apparently rapidly growing type should not be misinterpreted for a wall of effective tissue reaction but rather is a massive compression and degeneration of adjacent musculature.

Metastases. In contrast with previous observations, the incidence of metastases in our series was not more than 15 to 20 per cent. These were seen most commonly in the lungs and less so in the liver. In all instances but one, the metastases were associated with rapid growth. It is of interest to note that the dermis and subcutaneous space of the chicken has numerous vascular plexuses of considerable size. Thus, subcutaneous or intradermal inoculation may result in more frequent metastasis. Nevertheless, in our experience, the incidence of distant spread in intramuscular inoculation was still less than in previous reports.

Histogenesis. The histogenesis of the tumor is still not a settled problem. It is held by most that it is a connective tissue sarcoma. Carrel's[12, 13] and Roskin's[7] studies suggest that it is related to the reticulo-endothelial system. The evidence of phagocytosis by tumor cells supports this. The resemblance to human rhabdomyosarcoma or myoblastoma is striking except for the absence of any cross-striations or centrioles. The nature of the sarcolemma is still not settled by histologists. Possibly, the elucidation of this problem may throw light on the tissue origin of the Rous sarcoma.

PATHOLOGY OF THE TREATED BIRDS—*General Considerations.* The experiments described by Little *et al.*[11] should be viewed in the light of the following observations: (1) The effectiveness of the chemicals used to inhibit tumor growth is contingent with its continued administration. In some of the experiments, the birds were examined after a prolonged period during which treatment was discontinued. There was recrudescence of growth with a final poor result. (2) Delayed treatment, until a tumor is well developed, is ineffective or slightly so. (3) Highly effective tumor inhibitors were associated with toxic effects characterized by malnutrition, underdevelopment, or outright lethal action (amino folic acid in baby chicks). No specific morphological changes attributable to this toxic action were found in the viscera. The effect on the hematopoietic and central nervous systems is being explored now. (4) In many instances, notably in the case of baby chicks placed on a folic acid deficient diet, there were no traces of tumor or even of a residual inflammatory reaction at the site of inoculation. We believe that this complete, albeit temporary, inhibition is valid because of the great susceptibility of baby chicks to the tumor. (5) The animals in this study represent but a sample of a considerably larger number observed. The findings in the whole group and those of the pathological study are substantially in accord. (6) Except for the baby chicks on a folic acid deficient diet, where tumor inhibition was complete for the period of observation, and for baby chicks on the same diet with folic acid supplement, where all animals showed early and maximal tumor growth, the effectiveness of a given chemical was not uniform. In each experiment, there were examples that did not conform to the general pattern of the group. Factors which may account for this variation are: (a) dosage, (b) delay in treatment after inoculation, (c) discontinuance of treatment, (d) toxic effects, (e) age of animals, (f) natural resistance, and (g) technical difficulties. There are, undoubtedly, other factors at present unknown to us. With baby chicks of definitely known age and lineage, the results were much more clear-cut and uniform.

The chief manifestations of effective treatments were: (1) Complete absence of tumor. The time of observation was usually over 20 days, a period beyond the expected in which the animals should have well-developed tumors or even be dead as result. This was especially true of baby chicks. (2) Fewer and slower growing tumors. These were similar, grossly and microscopically, to the tumors of naturally resistant animals, but with the following differences. The extent of hydropic degeneration was considerably greater without corresponding organization and differentiation. Lymphocyte response was not as pronounced. This degeneration was seen at the periphery as well as in the center and, therefore, apparently had no relation to ischemia. (3) The almost complete lack of metastases. This must be qualified by the observation that, in baby chicks, direct tumor extension into the subjacent liver was rather common. We believe this to be due to the more infiltrative character of the tumor in the young, or to technical difficulties of injection in a small animal, or both.

DISCUSSION. Most of the above observations of the untreated bird are not original contributions to the pathology of Rous sarcoma. They have been made before by various observers, although often incompletely or in static terms. We feel justified in presenting them here because they represent the yardstick by which effectiveness of treatment in terms of pathology was measured.

Hemorrhage, infarction, and ischemia, with associated cellular death, degeneration, and inflammatory response are prominent features of rapid growth. Of these, central ischemia is the most constant. In contrast, slow growth in the resistant animal is characterized mainly by reduced cellular division, mitotic or amitotic; differentiation of the tumor cells into mature fibrocytes with collagen; and cellular degeneration and organization-repair with or without scarring.

The whole range of individual gross and microscopic appearances was seen in both the treated and untreated birds. Careful comparisons were made and no distinctive pattern or cytological detail could be found which was specific to either of the two groups. Yet, there is the impression that the inhibited tumor in the treated series shows much more cytological degeneration and, possibly, disruption of its mitotic figures. This raises the question—Is effective treatment an interference with the metabolism of the tumor cell? This problem is being explored further.

Our study emphasizes one factor in the biology of tumors, namely, the nutritional status of the host. The use of folic acid, its derivatives and antagonists, offers a valuable tool in the study of this problem.

SUMMARY AND CONCLUSIONS

(1) Approximately 350 birds, part of a considerably larger observed group, were inoculated with Rous chicken sarcoma, treated with folic acid, its derivatives, or a folic acid free diet.

(2) The morphologic features of the tumor in its life history and natural variations in the untreated bird are described and compared with those of treated animals.

(3) Certain of the folic acid derivatives completely or partly inhibit tumor growth, but may be associated with toxic effects.

(4) A folic acid free diet protects baby chicks against the tumor.

(5) The above actions are contingent on early and continued treatment.

(6) The addition of folic acid to a folic acid free diet materially increases the incidence and apparently enhances the growth of the tumor.

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THE EFFECT OF FOLIC ACID AND ITS ANTAGONISTS ON ROUS CHICKEN SARCOMA

By PAUL A. LITTLE, ANGUS SAMPATH, V. PAGANELLI, E. LOCKE, AND Y. SUBBAROW *

Introduction. Rous chicken sarcoma is a virus disease which has been extensively investigated since its discovery by Rous (1910). Peacock (1946) has recently evaluated the various findings concerning virus-induced fowl sarcomas and the chemically induced fowl sarcomas and has concluded that there is no reason to postulate a common etiology. Dr. Woll, in the preceding paper, has studied the pathology of the virus-induced sarcoma which is the subject of this report and has presented his findings.

In our laboratory, the growth of chicken tumors induced by virus inoculation has been completely prevented by maintaining the birds on a synthetic, folic acid free diet. Baby chicks were found to be more satisfactory for this demonstration than six weeks old birds in that the tumors were prevented in only forty per cent of the older birds and in a hundred per cent of baby chicks. We have also been able to prevent the growth of chicken tumors by use of chemical antagonists of folic acid.

Our exploratory studies consisted of experiments with four to nine weeks old New Hampshire Red chickens. A most valuable result of these experiments was the collection of data showing the uniformly high susceptibility of the birds used. The birds were treated from the time of first appearance of breast nodules until it was found that the number of naturally resistant birds encountered had not been more than ten per cent. Over three hundred birds were used. This high degree of success in inducing chicken tumors by virus inoculation may possibly be due to our choice of birds.

Selection of Chickens. In planning this work, we selected a single flock from several flocks of the New Hampshire Red variety that are maintained by Mr. Peter Pulis at Spring Lake Farm, Wyckoff, New Jersey. We had previously found that two of these flocks regularly produced chicks which were resistant to inoculation with Newcastle virus to the extent of 25 to 50 per cent even though there had been no history of the disease in them. We had found that two other flocks regularly produced chicks which were susceptible to inoculation with Newcastle virus to the extent of 90 to 100 per cent. A Newcastle virus susceptible flock was selected as the source of all of the birds used in our studies of chicken tumor. The four to nine weeks old birds were raised for us in flocks of three hundred and supplied in groups of fifty. The baby chicks were delivered to us when one day old. As many as two hundred baby chicks were employed in a single test. Day-old chicks were used for studies of the effect of chemical with synthetic diet. Five to six day old chicks were used for studies of the more toxic folic acid antagonists with ordinary diet.

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Method of Inoculation. The virus used in our experiments was prepared from selected breast tumors produced by inoculating four to nine weeks old birds in the pectoral muscle with one milliliter amounts of Rous sarcoma virus. The virus was originally obtained from Dr. James B. Murphy of the Rockefeller Institute. Birds having firm, well-developed tumors were sacrificed and the infected tissues were removed, cut into cubic centimeter pieces, placed in sterile vials, and stored in a dry-ice chest until used. The suspensions of virus used for inoculation consisted of the tissue from one vial dispersed with the aid of a Ten Broeck grinder in 100 milliliters of cold, sterile, distilled water. Inoculations were made within an hour from the time that the suspensions were prepared.

Since baby chicks were used in collecting most of the data presented at this time, their method of inoculation will be described in detail. The chicks were injected in the right breast with 0.2 milliliter of standardized suspension of tumor tissue after removing the feathers from the breast area. The injection was subcutaneous and inflated the skin over the whole area of the right breast until the material was absorbed. The point of entry used in making the injection was standardized and was located within 0.3 centimeter of and at half the length of the sternum. Groups of ten chicks were used for each determination and for untreated controls. Groups were treated by daily intraperitoneal injections of chemical starting on the day on which the virus was inoculated.

Special Conditions for Various Purposes. In our experiments, we have employed special conditions for various purposes. As previously stated, we treated four to nine weeks old birds from the time of first appearance of breast nodules for the purpose of eliminating naturally resistant birds. When it was found that we could depend on the high susceptibility of the birds used, we treated the birds from the time of inoculation of virus. Our first demonstration of the inhibitory effect of synthetic, folic acid free diet was made with six weeks old birds, which were fed the diet from the time of inoculation of virus. A group of ten birds which received the diet alone was resistant to inoculation to the extent of forty per cent, whereas another group of ten which received 0.1 milligram per cent of folic acid in the diet was resistant to the extent of only ten per cent. Baby chicks were found to be more satisfactory for this demonstration, possibly because they have a less highly developed microbial flora of crop and intestine. Oleson and co-workers (1946) have described the effect of the diet used by us on day-old white Leghorn chicks. For our purpose, it was not necessary to maintain the chicks on this diet for longer than three weeks.

Because of ease of handling, baby chicks were found to be useful in making quantitative studies, except in the case of a particular chemical, 4-amino-folic acid, which proved to be highly toxic. The activity of this chemical was best demonstrated by use of the older birds.

For the purpose of demonstrating the virus-stimulating effect of certain chemicals, baby chicks were maintained on synthetic, folic acid free diet and injected daily with the chemical. For the purpose of demonstrating the virus-inhibiting effects of other chemicals, baby chicks were maintained on ordinary diet which regularly permitted ninety per cent of untreated birds to develop tumors.

The Virus-Stimulating Effect of Chemicals. TABLE 1 illustrates the results of tests in which baby chicks were maintained on synthetic, folic acid free diet for the purpose of determining the virus-stimulating effect of chemicals. The method of determining and recording the different stages of development of breast tumors was based upon our knowledge of the maximum capacity of the breast. It was found that various workers could make essentially similar observations after

TABLE 1

PROTOCOL OF A BABY-CHICK TEST USING A SYNTHETIC, FOLIC ACID FREE DIET

Bird No.	Selected results from the record of daily observations				
	8th Day	10th Day	12th Day	14th Day	16th Day
Effect of synthetic, folic acid free diet alone					
2348	....	....	....	....	....
2349	....	....	....	....	....
2350	....	....	....	....	....
2351	....	....	....	....	....
2352	....	....	....	....	....
2353	....	....	....	....	....
2354	....	....	....	....	....
2355	....	....	....	....	....
2356	....	....	....	....	....
2357	....	....	....	....	....
Per cent	0	0	0	0	0
Effect of injecting 100 micrograms/day of folic acid					
2360	....	....	....	....	....
2361	?	P	2P	3P	4P
2362	....	....	....	?	P
2363	....	P	P	2P	3P
2364	....	P	2P	3P	Dead
2365	?	2P	2P	3P	4P
2366	....	P	2P	3P	4P
2367	....	P	2P	3P	4P
2368	?	2P	2P	3P	4P
2369	....	P	2P	3P	4P
Per cent	30	80	80	90	90

P: palpable swelling.
2P: ¼ to ½ full size.
3P: ½ to ¾ full size.
4P: full size.

TABLE 2

RESULTS OF BABY-CHICK TESTS USING A SYNTHETIC, FOLIC ACID FREE DIET

Chemical injected	Micrograms per day	Total chicks	Per cent chicks with tumor
None (four tests)	...	40	0
Folic acid	500	10	80
	100	10	90
	10	10	60
Folic acid with d(− −) glutamic acid	100	10	0
	10	10	0
N-methyl-folic acid	100	10	80
Pteroyldiglutamic acid	500	10	90
Pteroyltriglutamic acid	500	10	100
Pteroylaspartic acid	100	10	0
Pteroic acid	100	10	0
N-methylpteroic acid	100	10	0
P.A.B.-triglutamic acid	100	10	0

a preliminary inspection of a series of tumors in different stages of development.

The earliest evidence of tumor usually consisted of a slight elevation of the lower margin of the breast. This symptom was noted, in the record of results, as "questionable." Thereafter, almost daily changes occurred and were recorded in the following manner:

Symbol	*Meaning*
P	Palpable swelling
2P	Tumor ¼ to ½ full size
3P	Tumor ½ to ⅔ full size
4P	Tumor full size

Observations were made daily, beginning with the eighth day and continuing until the differences had been clearly demonstrated, at which time the birds were sacrificed and examined for internal evidence of tumor. The pathological studies were made by Dr. E. Woll.

As demonstrated in TABLE 1, the synthetic, folic acid free diet completely prevented the occurrence of breast tumors in the baby chicks, but nearly all of the chicks developed tumor when injected daily with folic acid. This result was regularly confirmed in the different tests in which we studied the virus-stimulating effect of other chemicals.

TABLE 2 shows the result of giving baby chicks, maintained on synthetic, folic acid free diet, daily injections of chemicals which are closely related to folic acid. It will be noted that folic acid with d(—) glutamic acid had no virus-stimulating effect. L(+) glutamic acid is the naturally occurring form found in the body and in the vitamin. N-methyl-folic acid, pteroyldiglutamic acid, and pteroyltriglutamic acid all contain the natural form of glutamic acid and showed as great a virus-stimulating effect as folic acid. Pteroylaspartic acid, pteroic acid, N-methylpteroic acid, and P.A.B.-triglutamic acid had no virus-stimulating effect. Chemicals which will be dealt with subsequently as folic acid antagonists also had no virus-stimulating effect.

This part of our report would not be complete without presentation of the formula of the synthetic, folic acid free diet used. As shown in TABLE 3, this diet contained adequate amounts of the thirteen vitamins which, together with folic acid, are required for complete nutrition of the chicken. Oleson and associates (1946) have described the effect of this diet on the feathering of chickens.

TABLE 3

FORMULA OF THE SYNTHETIC, FOLIC ACID FREE DIET USED IN CHICK TESTS

(This diet was made up by Dr. J. J. Oleson of our laboratories)

Cerelose	53.0%
Alc. ext. casein	22.0%
Salt mixture	4.3%
Calcium gluconate	3.0%
Gelatin	8.0%
Ruffex	4.0%
Soybean oil	5.0%
Cholic acid	0.25%
Cystine	0.45%
Choline chloride	200 mg. %
Calcium pantothenate	3 mg. %
Nicotinic amide	3 mg. %
Pyridoxine	0 5 mg. %
Thiamin chloride	0.3 mg. %
Biotin	0.03 mg. %
Riboflavin	0.5 mg. %
Inositol	100 mg. %
P.A.B.	5 mg. %
Vitamin E	5 mg. %
Vitamin K	0.2 mg. %
Vitamin A	3,500 units %
Vitamin D	200 units %

The Virus-Inhibiting Effect of Chemicals. TABLE 4 illustrates the results of tests in which baby chicks were maintained on a diet of ordinary chick-feed for the purpose of determining the virus-inhibiting effect of chemicals. The standard brand of prepared feed contained alfalfa meal, corn gluten meal, wheat bran, distillers dried solubles, dried buttermilk, fish meal, meat scrap, ground yellow corn, bone meal, pulverized oats, wheat middlings, soy bean oil meal, ground limestone, iodized salt, fortified vitamin A and D feeding oil, vitamin D-activated animal sterol, manganese sulphate, and riboflavin supplement.

TABLE 4

PROTOCOL OF A BABY-CHICK TEST USING A DIET OF ORDINARY CHICK-FEED

Bird No.	Selected results from the record of daily observations				
	12th Day	14th Day	16th Day	18th Day	20th Day
Effect of ordinary diet alone					
1171	2P	3P	4P	4P	4P
1172	P	2P	2P	3P	3P
1173	2P	4P	4P	4P	4P
1174	P	3P	3P	4P	4P
1175	....	P	P	3P	3P
1176	P	2P	3P	4P	4P
1177	....	P	2P	3P	3P
1178	....	....	....	....	....
1179	....	P	3P	4P	4P
1180	2P	4P	4P	4P	4P
Per cent	60	90	90	90	90
Effect of injecting 400 micrograms/day of 4-amino-pteroylaspartic acid					
1240	....	....	....	....	....
1241	....	....	....	....	....
1242	....	....	....	P	P
1243	....	....	....	....	....
1244	....	....	....	....	P
1245	....	....	....	....	....
1246	....	....	....	P	2P
1247	....	....	....	....	....
1248	....	....	....	....	P
1249	....	....	....	....	....
Per cent	0	0	0	20	40

P: palpable swelling.
2P: ¼ to ½ full size.
3P: ½ to ⅞ full size.
4P: full size.

TABLE 5

RESULTS OF BABY-CHICK TESTS USING A DIET OF ORDINARY CHICK-FEED

Chemical injected	Micrograms per day	Total chicks	Per cent chicks with tumor		
			12th Day	16th Day	20th Day
None (four tests)	....	40	52	87	95
4-amino-pteroylaspartic acid	400	10	0	0	40
	200	10	0	30	40
	100	10	0	30	60
	20	10	0	60	60
4-amino-folic acid with d(—) glutamic acid	400	10	0	0	0
	200	10	0	10	50
	100	10	0	30	40
	20	10	0	40	60
4-amino-N-methyl-folic acid	100	10	30	60	70
4-amino-N-methyl-pteroic acid	100	10	40	50	60
Pteroylaspartic acid	100	10	30	80	90
Folic acid with d(—) glutamic acid	400	10	70	80	90

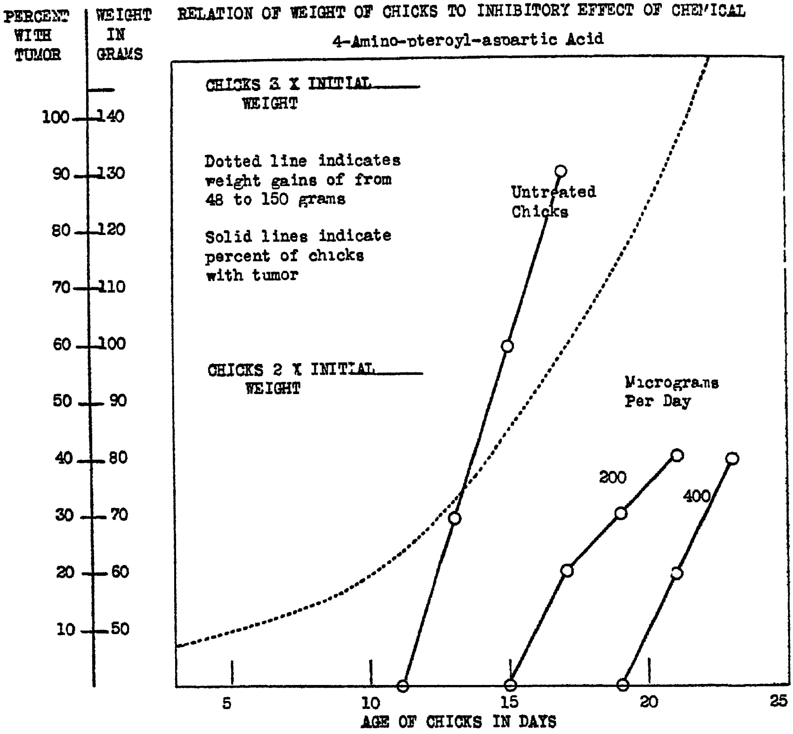

FIGURE 1.

As demonstrated in TABLE 4, nearly all of the chicks developed tumor when maintained on this diet. This result was regularly confirmed in the different tests in which we studied the virus-inhibiting effect of chemicals such as 4-amino-pteroyl-aspartic acid. The effect of injecting 400 micrograms per day of this chemical is seen to consist of a long delay in the appearance of any tumor and a marked decrease in the number of birds developing tumors.

TABLE 5 shows some of the quantitative data which we have collected on the virus-inhibiting effect of 4-amino-pteroylaspartic acid, 4-amino-folic acid with d(—) glutamic acid, and other chemicals of interest here. By comparing the percentage of birds exhibiting tumor on the 12th, 16th, and 20th day, we observe that there is a relationship between the amount of chemical injected, the time of appearance of any evidence of tumor, and the number of birds developing tumor. Doses of 100 micrograms of 4-amino-pteroylaspartic acid and 4-amino-folic acid with d(—) glutamic acid are seen to be more active than similar doses of 4-amino-N-methyl folic acid, 4-amino-N-methylpteroic acid, and pteroylaspartic acid. Doses of 400 micrograms of folic acid with d(—) glutamic acid are seen to be inactive. Sulfadiazine was also tested at doses of one milligram because of its well-known ability to antagonize a part of the folic acid molecule, and was found to be completely inactive.

The Problem of Dosage. As shown in

FIGURE 1, the inhibitory effects which we have demonstrated were undoubtedly diminished by the fact that the chicks were constantly growing. FIGURE 1 shows the daily gains in weight of the treated chicks and the results of daily injections of fixed doses of 400 and 200 micrograms of 4-amino-pteroylaspartic acid. By the 16th day, the chicks had increased to twice, and by the twenty-second day to three times their original weight. Probably the use of doses of chemicals based on milligrams per kilogram of body weight would be a more effective procedure.

The Toxicity and Virus-Inhibiting Effect of 4-Amino-Folic Acid. In its virus-inhibiting effect, another chemical, 4-amino-folic acid with 1(+) glutamic acid, was found to be even more active than either 4-amino-pteroylaspartic acid or 4-amino-folic acid with d(—) glutamic acid. Its virus-inhibiting effect was repeatedly demonstrated in our experiments with four to nine weeks old chickens, not only when treatment was started from the time of inoculation, but also when treatment was started after the sarcoma had started to grow and breast nodules had formed.

TABLE 6 shows the results of a comparative study of the activity of 4-amino-folic acid with 1(+) glutamic acid and

TABLE 6

PROTOCOL OF A TEST OF FOLIC ACID ANTAGONISTS USING SEVEN WEEKS OLD CHICKENS

Bird No.	Selected results from the record of daily observations				
	8th Day	10th Day	12th Day	14th Day	16th Day
Effect of ordinary diet alone (detail omitted)					
Per cent	40	70	100	100	100
Effect of injecting 1 milligram/day of 4-amino-pteroylaspartic acid					
K23	sw	sw	pale	blue	large
K24	?	sw	blue	large	large
K21	....	sw	blue	large	large
K28	....	sw	blue	large	large
K29	....	sw	pale	blue	large
K30	....	sw	pale	blue	large
K25	....	sw	pale	blue	large
K22	....	sw	blue	large	large
K26	....	....	....	....	....
K27	....	....	....	....	....
Per cent	20	80	80	80	80
Effect of injecting 1 milligram/day of 4-amino-folic acid					
K11	....	....	....	....	....
K12	....	....	....	....	....
K14	....	....	....	....	....
K15	....	....	....	....	....
K18	....	....	....	....	....
K19	....	....	....	....	....
K13	....	....	....	....	sw
K17	....	....	....	sw	pale
K16	....	....	....	sw	pale
K20	....	....	sw	pale	blue
Per cent	0	0	10	30	40

sw: palpable swelling.
pale: slight blue discoloration.
blue: brilliant blue discoloration.
large: full size.

4-amino-pteroylaspartic acid. The test was made using seven weeks old chickens weighing about a pound and a half. The birds were given daily doses of 1 milligram of chemical from the time of inoculation with virus. The virus-inhibiting effect of this amount of 4-amino-pteroylaspartic acid was scarcely measurable while that of 4-amino-folic acid was clearly demonstrated. As shown by the use of the words "pale" and "blue" in the record of results, a prominent characteristic of the breast tumors in adult birds was the change of color from normal to brilliant blue.

It is of interest that the greater activity of 4-amino-folic acid was associated with a greater toxicity. Continuous injections of 1 milligram per day produced noticeable emaciation in the adult birds. The injection of a few doses of 10 micrograms killed baby chicks. Dr. J. J. Oleson of our laboratory found that at least 25 milligrams of folic acid were required to partially neutralize the toxicity of 1 mg. of the 4-amino-folic acid for rats. In an attempt to demonstrate the virus-inhibiting effect in baby chicks, we found that pteroyldiglutamic acid and pteroyltriglutamic acid also partially neutralized the toxicity of 4-amino-folic acid when used in the 25 to 1 ratio. Such mixtures showed no virus-inhibiting effect.

Discussion of Results. It has been repeatedly observed by us and various other groups that folic acid in the amounts given has by itself caused no induction of tumors. The particular activity of folic acid which we have described was exhibited only after the virus-infected cells had been transplanted. Also, in adult birds, even after the sarcoma has started to grow, anti-folic acids have prevented further growth; or, when sufficient numbers of cells have multiplied, the tumor growth can be inhibited. Thus, we were led to the tentative conclusion that, probably, the effect of these substances is on virus multiplication.

It is of interest that Waisman and associates (1945) found thiamine-deficient mice harder to infect with both Lansing poliomyelitis virus and Theiler's encephalomyelitis virus. It may be that virus, in contrast to bacteria, has to depend completely on the enzymatic systems of the host for its nutrition and multiplication. It may be that folic acid, in the case of Rous sarcoma virus, forms the prosthetic group of an enzyme vitally needed for the virus "metabolism." More examples of infection by virus and state of nutrition of the host will further clarify these points.

Summary

In our laboratory, Rous chicken sarcoma has been completely prevented by maintaining the birds on a synthetic, folic acid free diet from the time of inoculation with virus. Baby chicks were found to be more satisfactory for this demonstration than adult birds since the results were 100 per cent. Rous chicken sarcoma was also prevented by treating the birds with antagonists of folic acid (4-amino-folic acid, 4-amino-folic acid with d(—) glutamic acid, and 4-amino-pteroylaspartic acid). When treatment was delayed to permit sufficient development of cells, these antagonists of folic acid were able to prevent further growth of the sarcoma.

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USE OF FOLIC ACID DERIVATIVES IN THE TREATMENT OF HUMAN LEUKEMIA

By LEO M. MEYER*

Before embarking upon the presentation of my data, I wish first to acknowledge the active collaboration of Drs. Harold Fink, Norton Ritz, Manuel Rowen, and Arthur Sawitzky, without whose help their accumulation in so short a time would have been impossible. I also wish to emphasize the preliminary nature of this report, the purpose of which is to direct other investigators with similar interests into related channels. It is hoped that no misinterpretation of a panacea for leukemia or "cancer cure" is suggested by the few experiences which we have had in the last few months.

Doctors Boothe and Smith have reviewed the chemical nature and synthesis of some of the compounds which we have used, so that no repetition is necessary. Since the work is still current and not completely studied and analyzed, only a part of the data will be presented here.

Our first experience was with the use of teropterin or pteroyltriglutamic acid. Seven patients with chronic lymphocytic leukemia, two with chronic myeloid leukemia, two with multiple myeloma, and one with acute lymphoblastic leukemia were treated for periods of 4 to 10 weeks with doses ranging up to 200 mg. a day. A survey of the cases shows that, in chronic lymphocytic leukemia, no significant change occurred in the peripheral blood or bone marrow studies. In addition, no regression was observed in the size of the lymph nodes, spleen or liver. Clinically, however, most of the patients expressed an improvement in the sense of well-being, feeling stronger, and enjoying some increase in appetite. One striking case was noted in a man who was bed-ridden and emaciated. Following injections of teropterin up to 100 mg. b.i.d., he was able to get out of bed and began to gain in weight. He received no other supportive therapy. The patients with chronic myeloid leukemia had been in good condition, so that the clinical improvement was not so noticeable. No change in the peripheral blood or bone marrow was noted.

The most dramatic results were observed in the 2 cases with multiple myeloma. Patients who had been confined to bed, unable to move, and suffering considerable pain, were relieved of their symptoms in 2 to 3 weeks and were able to get out of bed. I should like to emphasize, however, that there were no changes in the bone marrow studies or skeletal x-rays in this group. The improvement was entirely subjective.

The patient with acute lymphoblastic leukemia deserves to be presented in more detail. E.T., an adult white female, age 55, was under my care for 6 weeks before her present admission to the New York Hospital. Her chief complaints were pallor, weakness, bleeding gums, and purpura. A diagnosis of acute lymphoblastic leukemia had been made. She was treated with a filtrate of potato juice and ascorbic acid, as suggested by Isaacs, but no clinical or hematologic improvement was noted. At the New York Hospital, her blood count disclosed 32,700 leucocytes, of which 77 per cent were lymphoblasts and 18 per cent mature lymphocytes. Patient was started on 20 mg. teropterin intramuscularly, daily. The dosage was increased up to 100 mg. daily and later to 100 mg. b.i.d. There was a period about 6 weeks after the institution of therapy when the leucocyte count dropped to between 14 and 20,000. The more significant change, however, was the gradual fall in the number of blast cells and the coincident rise in mature lymphocytes. This change in peripheral blood was reflected in the bone marrow. Here, the blasts fell from 31 to 17 per cent and the lymphocytes rose from 66 to 81 per cent. Clinically, the patient had an episode of otitis media which was controlled with the use of penicillin. Destruction of the drum occurred. There was also infiltration of both retinae, which reduced the patient's vision. However, the temperature gradually returned to normal and the patient became ambulatory. The spleen slowly enlarged and was felt 2 cms. below the left costal border. The final clinical picture suggested a chronic lymphocytic leukemia. The patient was allowed to go home for the Thanksgiving holiday; that weekend she had several attacks of severe left upper quadrant pain which were interpreted as due to splenic infarction. She expired in 24 hours and no autopsy was obtained. The last blood count was 55,000 leucocytes, of which 90 per cent were mature lymphocytes.

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* Kings County Hospital, Brooklyn, N. Y.

Dr. Franklin Miller, of Philadelphia, has given me permission to present briefly a summary of his experience with diopterin or pteroyldiglutamic acid, in a case of acute lymphoblastic leukemia. This patient was treated from February, 1946, to September, 1947, with brewer's yeast daily, and transfusions were made every 2 weeks. On the last date, the leucocytes rose from 10,000 to 34,000 and the blast cells increased from 15 to 80 per cent. The patient was started on 50 mg. of diopterin intramuscularly, daily. About 3 weeks later, the total white cell count fell to 5,000 per cu.mm. and the blasts were reduced to 8 per cent. The remainder of the cells were mainly mature small lymphocytes. The patient required less frequent transfusions and the temperature, which had risen when the leucocytosis occurred, came down to normal.

In the group of anti-folic acid compounds, the most significant results to be reported are those in 5 cases of acute leukemia, all of whom showed somewhat similar findings.

J.L. a white male, age 6, was admitted to the King's County Hospital because of poor appetite, fatigability, and frontal headaches of one week's duration. Physical examination showed multiple petechiae, cervical, axillary and inguinal lymphadenopathy, splenomegaly, and hepatomegaly. The blood picture was that of an acute myeloblastic leukemia.

On admission, the total white cell count was 500,000, of which 99 per cent were myeloblasts. An-Fol-R[*] was started in doses of 20 mg. intramuscularly, daily. Six days later, the dose was increased to 40 mg. The total leucocytes fell from the extremely high level to 7,500 and the blood picture changed from 99 to 2 per cent blasts, and segmented leucocytes rose to a peak of 70 per cent. At this time, there was a complete regression of nodes, liver, and spleen. It was during this period that the patient suffered a cerebral accident involving the pons. His condition was critical, but he survived; as a result, he became blind. The reduction in count was short-lived and white cells began to climb. The dose of An-Fol-R was doubled, but there was no improvement. The medication was changed and the patient was given 20 mg. of Met-Fol-B,[†] which was increased to 40 mg. daily and, finally, to 40 mg. b.i.d., but the total leucocytes continued to rise and blast cells reappeared to the extent indicated. The lymphadenopathy, splenomegaly, and hepatomegaly recurred. The bone marrow showed a slight reduction in blast cells but not comparable to that seen in the peripheral blood. When the total leucocytes rose again, the blast cells increased in the bone marrow.

H.J. a negro female, age 76, was admitted to the hospital because of extreme weakness. On physical examination, a large thyroid was noted. X-ray of the chest showed a substernal thyroid and large traction diverticulae of the esophagus. The blood picture was that of an acute myeloblastic leukemia. The total leucocytes were 150.000, with 68 per cent myeloblasts. Patient was placed on 40 mg. Met-Fol-B, daily. This was increased to

[*] Cryptic name for *N*-[-4- { [(2-amino-4-hydroxy-6-pteridyl)methyl]amino } -benzoyl]*d,l*-aspartic acid.

[†] Cryptic name for 4- { *N*-[(2-amino-4-hydroxy-6-pteridyl)methyl]-*N*-methylamino } benzoic acid.

b.i.d. in 3 days. The total leucocytes fell from 150,000 to 1,500 in 12 days. The blasts came down to between 40 and 52 per cent. The polymorphonuclears were only slightly increased to 8 per cent and mature lymphocytes rose at this time to 34 per cent. When the leucocytes fell to 1,500, the dosage was reduced to 20 mg. b.i.d. However, when the white blood count began to increase, the dose was raised to 40 mg. b.i.d. and then 80 mg. b.i.d., but this did not affect the rising count. With the increase in total leucocytes, the blast cells also rose in number. The bone marrow reflected the peripheral leucopenia in that the total nucleated cell count fell from 150,000 to 28,000 and the blasts from 80 to 46 per cent. Within 3 days, however, there was evidence of a rising total nucleated cell count and blasts.

About one week ago, the patient was started on a third anti-folic acid compound which appeared to be producing a second reduction in total cells. However, she died last Friday of her disease and the efficacy of this derivative could not be ascertained.

D.F., a negro boy, age 6, was admitted to the hospital because of weakness and severe migratory joint pains. A diagnosis of acute rheumatic fever was entertained, but the blood counts and bone marrow disclosed an acute myeloblastic leukemia. Except for the tender, painful joints and splenomegaly 2 cm. below the costal border, examination was not remarkable Several blood counts showed leucocytes ranging from 5 to 10,000. On the day treatment was started, the white blood cells were counted at 5,500 with 19 per cent myeloblasts, 3 per cent myelocytes, 8 per cent metamyelocytes, 9 per cent non-segmented neutrophiles, and 22 per cent segmented forms. Patient was given 20 mg. Met-Fol-B daily. This was increased to 20 mg. b.i.d. in 3 days, and 3 days later the dose was increased to 40 mg. b.i.d. There was a slight but persistent leucopenia which rose slowly again to a figure close to the pre-treatment level. However, the total number of immature cells was reduced to 8 per cent and the more mature forms were increased to 19 per cent bands and 36 per cent segmented forms. Except for a severe nose-bleed which the patient suffered around Nov. 23, the general condition has remained fair. The joint pains have disappeared and the spleen and liver are now palpable 2-3 cm. below the costal border.

M.M., an adult white male, age about 30, was admitted to the hospital from another institution with the diagnosis of acute myeloblastic leukemia. The patient's major complaints were weakness, pallor, and swollen, spongy gums of about 6 weeks' duration. The initial blood count showed 36,000 leucocytes of which 17 per cent were myeloblasts, 38 per cent myelocytes, and 16 per cent segmented and non-segmented forms. The patient was started on 20 mg. An-Fol-R daily. This dose was doubled 7 days later. 23 days after treatment was begun, we ran out of medication and the patient received no therapy for 3 days. Thereafter, he was given 40 mg. Met-Fol-B daily which was increased in 72 hours to 80 mg. a day. The most significant findings in his blood are the reduction in myeloblasts to 1 per cent from 17 per cent, myelocytes from 38 to 26 per cent, and an increase in segmented and non-segmented neutrophiles from 16 to 40 per cent. The changes in the total white cell count are not so striking as in the 3 previous cases, but this may be due to the fact that these counts were taken by different persons, and we are all aware of the errors inherent in such a procedure. However, all of the differential studies were made by one observer.

The bone marrow shows a similar trend. On admission, the blasts were 42 per cent, myelocytes 18 and segmented neutrophiles 2 per cent. One month after treatment, the blasts were reduced to 4 per cent, myelocytes increased to 67 per cent, and neutrophiles up to 17 per cent. Clinically, the patient feels much improved. In fact, early last month he wanted to leave the hospital to go bear-hunting. There has been considerable regression of the swelling of the gums, but they are by no means normal. During the past 2-3 weeks, the patient has had several attacks of bilateral peri-tonsillar abscesses which have been well controlled with pencillin.

R.L., an adult white male, age 21, was admitted to the hospital because of weakness and pallor of about 1 week's duration. Except for his pallor, the physical examination was not remarkable. The blood count revealed 5,000 white cells, of which 30 per cent were myeloblasts and 30 per cent myelocytes. He was started on 20 mg. An-Fol-R daily, which was increased in 2 weeks to 40 mg. After 1 month, the drug was discontinued for 3 days, following which Met-Fol-B therapy was begun, in doses of first 40 mg. and then 80 mg. daily. A review of the blood smears shows a significant reduction in the number of myeloblasts with an increase in the myelocytes. The number of mature polymorphonuclear leucocytes did not change. Again, the variations in total white cells may be due to the technique, as the counts were taken by different technicians. However, the differential smears were studied by one observer. In the bone marrow, the changes are more striking. The myeloblasts have been reduced from 49 to 1.5 per cent and the myelocytes increased from 22 to 82 per cent. Clinically, the patient has been critically ill for short periods, but each time he has recovered, due largely to transfusions and antibiotic therapy. Between infections—if these are infections—he appears quite well and is ambulatory. At present, he is very ill with a pneumonia which followed a severe sore throat.

In addition to these 5 patients with acute leukemia treated with anti-folic acid compounds, we wish to report that we have had 3 patients with chronic lymphocytic leukemia under our care for 4-6 weeks, using Met-Fol-B in doses of 40 mg. b.i.d., who have shown no hematological improvement. The patients state, however, that they feel better and enjoy a sense of well-being.

In summary, we have treated acute and chronic leukemia, as well as multiple myeloma, for varying periods of time, using derivatives of folic acid. In general, regardless of the compound, if treated for periods over 2-3 weeks, the patients appear to feel better, eat more, and in the cases of myeloma have relief from pain. In the cases of chronic leukemia and multiple myeloma, no alteration in blood picture or bone marrow took place. It has not been our experience to see frequent spontaneous reductions in the number of blast cells in acute leukemia. In the cases reported, using teropterin and diopterin, the lymphoblasts were replaced by mature lymphocytes. This is completely different from the reaction following radiation or nitrogen mustard therapy. With the antifolic acid compounds, there was a reduction in the total white cells in 3 cases, with a temporary shift to the right. This remission, though short-lived, suggested an alteration or interference with the metabolism or chemical reaction in the malignant cells. In 2 cases of acute leukemia, there has been a persistent shift to the right.

Again, I wish to emphasize the preliminary nature of this report. The purpose has been solely to acquaint other investigators with our findings, so that we may be of mutual help in our work. I believe that the data are suitable only for distribution to that portion of the medical profession actively engaged in this work.

DISCUSSION

By ALFRED GELLHORN*

In the series of papers presented here, the only objective evidence that folic acid has a carcinolytic effect, was, it seems to me, cited in the historical review by Dr. Waller. In this paper, the observations of Dr. Lewisohn and his associates at Mount Sinai Hospital were referred to. It will be recalled that these investigators found that protracted intravenous administration of folic acid resulted in the disappearance of spontaneous adenocarcinoma of the breast in mice in as high as 30 per cent of the experimental group. Dr. Waller failed to mention, however, that this experimental observation has not been confirmed although an extensive investigation by Dr. Sugiura at the Memorial Hospital was carried out in an attempt to verify it.

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Very briefly, I should like to comment on the clinical evaluation of teropterin in the therapy of human neoplastic disease. In Dr. Klainer's report of a single case of carcinoma of the breast with osseous metastases, it seems to me very important to note the therapy given in addition to teropterin. As I recall, the patient received 100-150 mg. of testosterone propionate daily for one month and, in addition, was surgically castrated. Adair and Hermann at the Memorial Hospital have reported the results of testosterone therapy in a large series of patients with just such lesions as existed in Dr. Klainer's case. Adair and Hermann found that relief of pain and, not infrequently, x-ray evidence of bone healing occurred in a large percentage of their patients following the administration of as little as 2400 mg. of testosterone propionate. The duration of remission induced by such androgen therapy was, in some instances, as long as eighteen months. I believe, therefore, that, in the evaluation of teropterin therapy in Dr. Klainer's patient, the fact should be stressed that she also received other forms of treatment which could be expected to produce the remission noted.

In the series of patients presented by Dr. Lehv, in only one possible instance was there objective evidence that a fundamental alteration of the course of the disease had been produced by teropterin therapy. This was the patient with carcinoma of the prostate, with evidence of neurologic involvement by metastases, who had an improvement of his neurological signs and symptoms following therapy with pteroyltriglutamic acid. The results, even in this case, were equivocal due to the fact that an orchidectomy had been performed prior to the administration of teropterin. In all the other patients, it appeared that the course of the disease had in no way been altered. Dr. Lehv stated that serial biopsies were taken on a number of the cases, but no mention was made of morphological changes in the neoplasms. It is, however, tremendously interesting that, in Dr. Lehv's patients as well as those of Dr. Meyer, a sense of well-being and an apparent analgesic effect were produced. In this connection, it is to be recalled that Dr. Waelsch and his associates at the Psychiatric Institute have shown that glutamic acid has an anticonvulsive effect in *petit mal* epilepsy and also favorably affects cortical function in feeble-minded children. This suggests the possibility that the subjective improvement noted by the patients on teropterin therapy was due to a central nervous system effect of the drug related to the pharmacological action of glutamic acid. Admittedly, the dosages of glutamic acid employed by Dr. Waelsch were of an entirely different order of magnitude than the amount present in teropterin; however, the peptide linkage of the glutamic acids in teropterin might alter its distribution and effect.

In the patients with leukemia treated by Dr. Meyer, it again appears that the course of the disease was not significantly altered by teropterin. To be sure, in several of these patients there was a change in the peripheral blood picture with a decrease in the number of blasts and other immature forms. However, this sort of change frequently occurs spontaneously in acute leukemia and has also been induced repeatedly with radiotherapy or nitrogen mustard. It has been demonstrated that these latter two therapeutic regimes do not favorably influence the outcome of the disease, and it does not seem to me that evidence has been presented which would indicate that teropterin has actually achieved more than a temporary effect.

REPORT OF THE ANNUAL MEETING

December 17, 1947

The 130th Annual Meeting of the Academy, for the election of Officers and Fellows, the presentation of reports, and the transaction of other business was held at the Hotel Astor on the evening of Wednesday, December 17, 1947.

The first order of business was the presentation for final vote of the revised Constitution of the Academy. This revision, which is intended to adapt the basic organization of the Academy to present conditions, was unanimously adopted in the form presented below.

THE CONSTITUTION OF THE NEW YORK ACADEMY OF SCIENCES

(as revised and adopted at the Annual Meeting of the Academy, December 17, 1947 effective as of this date).

ARTICLE I. The name of this Corporation shall be The New York Academy of Sciences. Its object shall be the advancement and diffusion of scientific knowledge, and the center of its activities shall be in the City of New York.

ARTICLE II. The Academy shall consist of six classes of members, namely: Life Members, Sustaining Members, Annual Active Members, Student Members, Honorary Life Members, and Corporation Members. The designation Active Member shall be applied to all voting members of the Academy which shall include all the above classes except Student Members, Honorary Life Members, and Corporation Members. Active Members of the Academy, outstanding in virtue of their scientific attainments, may be chosen to become Fellows of the Academy. Honorary Life Members shall be chosen from among persons who have attained distinction in some branch of science. The number of Honorary Life Members shall not exceed one hundred.

ARTICLE III. None but those Active Members of the Academy who have paid their dues up to and including the preceding fiscal year shall be entitled to vote or shall be eligible to hold office in the Academy.

ARTICLE IV. The officers of the Academy shall be a President, two Vice-Presidents, a Corresponding Secretary, a Recording Secretary, a Treasurer, an Editor, nine elected Councilors, a Finance Committee of three, and the Chairman of each established Section of the Academy. The annual election of these officers shall be held on the third Monday in December, or on some other day in December specifically designated by vote of the Council, the officers then chosen to take office on the first day of January following.

An Executive Director shall be elected annually by the Council, at its first meeting in January, for a term of one year, beginning with the first day of February following, or until his successor shall have been chosen.

ARTICLE V. The officers of the Academy named in Article IV shall constitute a Council, which shall be the executive body of the Academy with general control over its affairs, including the establishment, division, subdivision or discontinuance of Section of the Academy and the power to fill *ad interim* any vacancies in the offices. Past Presidents of the Academy shall be invited annually to serve as members of the Council and, upon their acceptance of the invitation, they shall be *ex-officio* members of the Council for the following calendar year and eligible for a renewal of such invitation by order of the Council.

ARTICLE VI. The Council shall have the power to establish or discontinue affiliation of scientific societies or associations, organized for the study of any branch of science, with The New York Academy of Sciences. Members of allied societies may become Active Members of the Academy upon election by the Council and upon payment of the fee prescribed for such membership, otherwise as members of an allied society they shall be entitled to such privileges as shall be determined by the Council of the Academy. Each allied society shall have the right to delegate one of its members, who is also an Active Member of the Academy, to the Council of the Academy, and such delegate shall have all the rights and privileges of other Councilors.

ARTICLE VII. The President, Vice-Presidents and Sectional Chairmen shall not be eligible to more than one re-election. The Secretaries, Treasurer, Editor and Members of the Finance Committee shall be eligible to re-election without limitation. The President, Vice-Presidents and Secretaries shall be Fellows. The terms of office of elected Councilors shall be three years, and these officers shall be so grouped that three, at least one of whom shall be a Fellow, shall be elected and three retired each year. Elected Councilors shall not be eligible to re-election until after the expiration of one year.

ARTICLE VIII. The election of officers shall be by ballot, and the candidates having the greatest number of votes shall be declared duly elected.

ARTICLE IX. Ten Active Members shall form a quorum at any meeting of the Academy at which business is transacted.

ARTICLE X. The Council shall establish such by-laws, not in conflict with the Constitution, as may be necessary for the proper government of the Academy and may amend them from time to time as therein provided.

ARTICLE XI. This Constitution may be amended by a vote of not less than three-fourths of the Active Members present and voting at a stated business meeting of the Academy, provided that the Recording Secretary shall send a copy of the proposed amendment, at least two weeks before the meeting, at which a vote shall be taken, to each Active Member entitled to vote.

REPORTS OF OFFICERS AND COMMITTEES

The Recording Secretary, in his report, drew the attention of the Members of the Academy to the time and energy absorbed in planning, preparation and organization, before the regular meetings and conferences of the Academy can be held with their extensive series of scientific reports and discussions. Likewise, after the meetings are over, there comes a difficult period of revising, assembling and editing the papers, in order to bring about publication and final distribution.

It would be difficult to list individually the participating scientists, the Committee and Council Members, as well as the Academy's employed staff, in acknowledging our indebtedness for their wholehearted and assiduous support of this Society. However, special thanks are due to those who have been outstanding in their services for making possible this successful year in the Academy's history.

Appreciation is expressed to Doctor Hans Molitor for his many services to the Academy not only as a Councilor but as Chairman of the Rules Committee while it was engaged in the study and revision of the Constitution of the Academy.

The Chairmen and Secretaries of the various Sections have maintained a high standard in selecting scientific material for the programs of the evening meetings.

Doctors Ross F. Nigrelli and Charles R. Schroeder, of the Section of Biology, have steered nine of the ten conferences, held this year, through to success. The officers of the Section of Psychology, Doctors S. Stansfeld Sargent and Clairette P. Armstrong, managed an outstanding conference in the field of clinical psychology.

Doctor Myron Gordon, Organizing Chairman of the Conference on Melanomas, not only has served as Consulting Editor of the volume now forthcoming on *"The Biology of Melanomas,"* but in addition has obtained $10,500 in five separate grants-in-aid toward the publication costs of this important work.

Through the efforts of Doctors Fredrick F. Yonkman, Carl A. Dragstedt and Y. SubbaRow, sufficient funds were secured to enable the Academy to bring to this country Sir Henry H. Dale, London, England; Doctor Daniel Bovet, Rome, Italy; Doctor M. Roche e Silva, Sao Paulo, Brazil; Doctor Rolf Meier, Basel, Switzerland; and Doctor E. T. Waters, Toronto, Canada, as well as the other speakers on the program of the Conference on *"Antihistamine Agents in Allergy."*

Thanks are due to Doctor Charles Glen King and Doctor Philip S. Owen for facilitating support for both the Conference and publication on *"Nutrition in Relation to Cancer,"* which was sponsored jointly by the Academy and the Panel on Nutrition, Committee on Growth, of the National Research Council.

We wish to acknowledge the hospitable cooperation of The New York Academy of Medicine through the courtesy of its President, Doctor George Baehr, and its Director, Doctor Howard Reid Craig, on the occasion of the Conference on *"The Adrenal Cortex,"* jointly sponsored by The New York Academy of Sciences and The New York Academy of Medicine, in recognition of the Centennial Celebration of the latter Society.

The titles of the seven additional Conferences held are as follows: Section of Biology—*"The Mechanics of Development," "Thyroid Function as Disclosed by Newer Methods of Study," "Allergy," "Structure in Relation to Cellular Function," "The Chemotherapy of Filariasis,"* and *"Medical Mycology"*; Section of Psychology—*"Current Trends in Clinical Psychology."*

Nine Life Members, 22 Sustaining Members, 1,045 Annual Active Members, 47 Associate Members and 43 Student Members were added to the rolls, of which 1,114 are now in good standing and 52 await qualification through payment of dues. Thus, a total of 1,166 new Members was added during the year. One Annual Active Member was transferred to Life Membership by payment of the Life Membership fee. Fifty-four transfers and 21 reinstatements were also effected between the various classes of Membership, with the approval of the Council.

The Academy has lost by death 5 Honorary Life Members, 4 Life Members, 5 Sustaining Members, 10 Annual Active Members, and 5 Associate Members. Sixty-two resignations were accepted, 57 names were dropped for non-payment of dues, and 3 for not having qualified for membership.

The record now stands with a net gain of 1,035 Members. There are, at present, upon the rolls of the Academy 4,009 Members, of whom 456 are Fellows.

The report of the Corresponding Secretary showed that, due to the death of five Honorary Life Members, recorded with regret, the number upon the rolls of the Academy was reduced from 49 to 44 during the past year.

The Editor reported that, during the fiscal year of 1947, the Academy published a total of 1,918 pages. Of these, 1,098 pages appeared in the Annals (8 articles) and 478 pages in the Special Publications, including 87 papers by 118 authors. A total of 342 pages were published in the Transactions. In addition to the Annals published this year, Article 2 of Volume 49 is in press, to be issued shortly. A number of other articles are in process of editing and will be published during the coming year in Volumes 49, 50, and 51. Volume 17, Part I of the Scientific Survey of Porto Rico and the Virgin Islands is also in the Editor's hands and will be issued soon.

The detailed list of those publications which have appeared this year is as follows:

ANNALS

Volume 46, Article 10, *"Brain and Body Weight in Man"* (1 paper), by Earl W. Count. Pages 993–1122. Published January 15, 1947.

Volume 47, Article 5, *"Physiological and Psychological Factors in Sex Behavior"* (6 papers), by S. Bernard Wortis, Gregory Bateson, William E. Galt, Morris Herman, Alfred C. Kinsey, and William C. Young. Pages 603–664. Published May 9, 1947.

Volume 47, Article 6, *"Muscular Contraction"* (12 papers), by Alexander Sandow, R. S. Bear, Ernst Fischer, A. S. Gilson, Jr., Eugene Guth, C. E. Hall, M. A. Jakus, S. W. Kuffler, Otto Meyerhof, Severo Ochoa, Robert W. Ramsey, F. O. Schmitt, G. M. Schoepfle, Shih-Chang Shen, H. Burr Steinbach, and S. M. Walker. Pages 665–930. Published May 30, 1947.

Volume 48, Article 5 (Supplement), *"The Synthesis of Pteroylglutamic Acid (Liver* L. Casei *Factor) and Pteroic Acid—Part II"* (1 paper), by M. E. Hultquist, Robert B. Angier, James H. Boothe, Donna B. Cosulich, M. J. Fahrenbach, Brian L. Hutchings, Erwin Kuh, John H. Mowat, E. H. Northey, Doris R. Seeger, Joseph Semb, J. P. Sickels, James M. Smith, Jr., E. L. R. Stokstad, Y. SubbaRow, and Coy W. Waller. Pages i–vi. Published October 3, 1947.

Volume 48, Article 6, *"The Relation of Diseases in the Lower Animals to Human Welfare"* (10 papers), by William A. Hagan, Herald R. Cox, William H. Feldman, I. Forest Huddleson, Harald N. Johnson, Raymond A. Kelser, Joseph V. Klauder, Karl F. Meyer, C. D. Stein, and Willard H. Wright. Pages 351–576. Published April 10, 1947.

Volume 48, Article 7, *"Some Aspects of Red Cell Production and Destruction"* (6 papers), by Eric Ponder, William B. Castle, Harry A Charipper, William Dameshek, Albert S. Gordon, S. Granick, and F. S. Robscheit-Robbins. Pages 577–704. Published July 3, 1947.

Volume 48, Article 8, *"Convection Patterns in the Atmosphere and Ocean"* (11 papers), by R. B. Montgomery, John C. Armstrong, Horace R. Byers, Phil E. Church, Richard A. Craig, B. Haurwitz, Walter H. Munk, Henry Stommel, Harry Wexler, Raymond Wexler, A. H. Woodcock, and Jeffries Wyman. Pages 705–844. Published September 15, 1947.

Volume 49, Article 1, *"Nutrition in Relation to Cancer"* (12 papers), by Charles Glen King, John J. Bittner, D. H. Copeland, R. W. Engel, C. J. Kensler, George W. Kidder, G. Burroughs Mider, J. A. Miller, Harold P. Morris, William J. Robbins, W. D. Salmon, Albert Tannenbaum, E. L. Tatum, Florence R. White, Julius White, and Philip R. White. Pages 1–140. Published September 7, 1947.

SPECIAL PUBLICATIONS

Volume IV, *"The Biology of Melanomas"* (28 papers), by Myron Gordon, Glenn H. Algire, S. William Becker, Harold F. Blum, Liane R. Brauch, E. A. Sheremetieva-Brunst, V. V. Brunst, Dean Burk, Gladys Cameron, Graham Philips DuShane, Frank H. J. Figge, Clara E. Fisher, Denis L. Fox, Samuel A. Goldberg, C. G. Grand, Jesse P. Greenstein, James B. Hamilton, R. G. Harrison, Marie L. Hesselbach, M. J. Kopac, Frances Y. Legallais, Michael Levine, Eleanor J. Macdonald, Madge Thurlow Macklin, Howard S. Mason, P. Masson, J. M. Odiorne, George T. Pack, Elizabeth S. Russell, W. L. Russell, Leonell C. Strong, Kanematsu Sugiura, Helenor Campbell Wilder, and B. H. Willier. Pages i–xii, 1–466. This is now being printed as a cloth-bound volume, and will appear about January 1, 1948.

TRANSACTIONS

Series II, Volume 9, Nos. 1–8, consisting of 342 pages, was completed, printed and distributed each month from November, 1946, to June, 1947, inclusive.

The Librarian reported that, during the year 1947, the Academy distributed 74,560 separate publications and 6,788 complete volumes of the various published series, as follows:

Separate Numbers—*Annals,* 64,111; *Scientific Survey of Porto Rico and the Virgin Islands,* 415; *Transactions,* Series II, 7,023; *Memoirs,* 49; *Bulletins,* 2,962.

Volumes—*Annals,* 376; *Annals of the*

Lyceum, 1; *Scientific Survey of Porto Rico and the Virgin Islands,* 171; *Special Publications,* 1,065; *Transactions,* Series I, 145, and Series II, 115; *Bulletins,* 4,915.

During the present year, ten complete sets of the publications of the Academy were sent to the Philippines and China to aid in rebuilding institutional libraries destroyed during the war. This brings the total to 18 sets donated by the Academy for this purpose. In addition, a complete set of our publications has been presented to the Library of the United Nations Organization. The institutions, throughout the world, with which the Academy exchanges or in which it deposits its publications, have now been brought up to date in this respect.

The Academy has received, from its exchange institutions, 2,415 separate publications, which have been duly turned over to the Library of The American Museum of Natural History, in accordance with the agreement between the two institutions.

The total value of publications distributed gratis to the members, authors and exchange institutions is $40,326.43.

The Treasurer reported that the Academy, while pursuing its established program of progressive activities in the field of science, also exhibits a corresponding financial development, continuing the progress of recent years. During the fiscal year ending November 30, 1947, receipts from all sources totaled $84,611.57. The disbursements amounted to $77,042.59.

New Members elected during the year totaled 1,166, in addition to 21 reinstatements. The receipts from this source include $1,000.00 for 10 Life Memberships which was added to the Permanent Fund, thus increasing the endowment of the Academy to that extent.

Receipts from sales of publications and library exchanges amounted to $25,247.64.

Mr. William Otis Sweet contributed $1,000.00 to be added to the fund established in his name, from the capital of which he also donated a grant-in-aid of $1,500.00, in support of the Academy's publication program.

The Treasurer also wishes to acknowledge with gratitude and appreciation, on behalf of the Academy, twenty-one donations or grants totaling $25,420.93, received during 1947, to aid in defraying both conference and publication expenses, as follows: American Cancer Society, $5,000; American Cyanamid Company, $3,400.06; Ciba Pharmaceutical Products, Inc., $400.00; Ciba Ltd., Canada, $100.00; Jane Coffin Childs Memorial Fund for Medical Research, $1,000.00; Anna Fuller Fund, $1,000.00; Hoffman-La Roche, Inc., $400.00; Merck and Company, $400.00; Department of Agriculture and Commerce, Puerto Rico, $3,000.00; National Cancer Research Institute, $3,000.00; National Research Council, Panel on Growth, $804.87; The Nutrition Foundation, Inc., $1,699.00; Parke, Davis and Company, $400.00; The Rockefeller Foundation, $1,500.00; E. R. Squibb and Sons, $400.00; William Otis Sweet, $1,000.00 (plus $500 transferred from the capital of the Sweet Fund); University of Puerto Rico, $1,500.00; The Upjohn Company, $400.00; sundry contributions for Publication Fund, $17.00.

The Finance Committee, acting in an advisory capacity to the Treasurer in the matter of investments, through the sales of certain stocks produced a net capital gain of $8,649.98. Of this amount, $5,135.23 was allocated to cover the cost of the preliminary survey made on behalf of the Academy by the firm of Tamblyn and Brown, Inc. The balance, $3,514.75, was used to aid in covering publication costs. The interest on investments amounted to $5,198.46, representing a yield of 5.6%.

The book value of the Academy's investments and cash on hand, as of November 30, 1947, now amounts to $104,376.39, as shown on the balance sheet.

By order of the Council, the books of

account have been checked and audited by a professional firm of auditors. The financial report of the Academy has been approved by the Finance Committee as provided in the By-Laws.

The property of the Academy was verified by the auditor and the Finance Committee, and the Treasurer's report examined and attested by the Finance Committee as of December 11, 1947.

AWARD OF A. CRESSY MORRISON PRIZES

The A. Cressy Morrison Prize of $500.00 for the best paper on the sources of solar and stellar energy was awarded to the paper entitled: *"Energy Sources in Giant Stars,"* by Donald H. Menzel, Harvard Observatory, Harvard University, Cambridge, Massachusetts.*

A prize of $200.00 for the most acceptable paper in a field of Natural Science covered by the Academy, or an Affiliated Society, was awarded to the paper entitled: *"The Effect of* Clostridium tetani *Toxoid in the Relapse of Malaria,"* by Eusebio Y. Garcia, Medical Research Clinic, Binan, Laguna, Republic of the Philippines.*

The Prize Committee takes pleasure in announcing, on behalf of Mr. Morrison, that he has again offered two prizes, of $200.00 each, for the two most outstanding papers in a field of science covered by the Academy or an Affiliated Society, to be competed for during the year 1948. Announcement of these awards will be made at the Annual Meeting of the Academy, to be held in December, 1948. Final announcement of the terms of this competition will be made in the February issue of the Transactions and special circulars will also be sent to members of the Academy and its Affiliated Societies.

ELECTION OF FELLOWS AND HONORARY LIFE MEMBERS

The following Members were elected to Fellowship:

Mark H. Adams, Ph.D.
Mildred Adams, Ph.D.
John A. Aeschlimann, Ph.D.
Turner Alfrey, Jr., Ph.D.
James B. Allison, Ph.D.
John C. Armstrong, Ph.D.
Lester R. Aronson
Reginald Auchincloss, Ph.D.
Robert Ballentine, Ph.D.
W. Dexter Bellamy, Ph.D.
Gerrit Bevelander, Ph.D.
Isabel H. Blount, Ph.D.
Raymond F. Blount, Ph.D.
Stuart R. Brinkley, Ph.D.
Alice I. Bryan, Ph.D.
Victor Carabba, M.D.
Benjamin W. Carey, M.D.
Robert Chambers, Ph.D.
George R. Cowgill, Ph.D.
F. J. Crane, D.Sc.
Harry E. Crum
Gaetano F. D'Alelio, Ph.D.
Charles R. Dawson, Ph.D.
Herman W. Dorn, Ph.D.
Fred W. Dunihue, Ph.D.
Max S. Dunn, Ph.D.
T. H. Eaton, Jr., Ph.D.
William F. Ehret, Ph.D.
Robert K. Enders, Ph.D.
Jackson P. English, Ph.D.
William O. Field, Jr., B.S.
Ella Fishberg, M.D.
Charles N. Frey, Ph.D.
James P. Fugassi, M.D.
Ellis J. Fulmer, Ph.D.
Arthur I. Gates, Ph.D.
Robert Gaunt, Ph.D.
Alfred Gellhorn, M.D.
A. E. Gessler, Ph.D.
Willis A. Gibbons, Ph.D.
Alfred Gilman, Ph.D.
Harry Gold, M.D.
Harry Goldblatt, M.D.
Max A. Goldzieher, M.D.
Louis S. Goodman, M.D.
Aubrey Gorbman, Ph.D.

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* For abstracts of both prize papers, see pp. 125-6 of this number of TRANSACTIONS. The paper by Dr. Garcia will be published, in full, as ANNALS, Volume 50, Article 2.

Sam Granick, Ph.D.
Roy O. Greep, Ph.D.
Arthur Grollman, Ph.D.
Rudolf E. Gruber, Sc.D.
Harry Grundfest, Ph.D.
Walter J. Hamer, Ph.D.
Ben King Harned, Ph.D.
Henry H. Hausner, D.E.
Roderick Heffron, M.D.
Harold E. Himwich, M.D.
Charles B. Hitchcock, M.A.
Hudson Hoagland, Ph.D.
Seymore Hochberg, Ph.D.
Jorgen Holmboe, M.S.
John C. Hostetter, Sc.D.
Seymour H. Hutner, Ph.D.
Carl Iddings, Ph.D.
Thomas H. James, Ph.D.
Herbert H. Jasper, Ph.D.
Thomas H. Jukes, Ph.D.
Francois Kertesz, Sc.D.
Roy H. Kienle, Ph.D.
Helen D. King, Ph.D.
Israel S. Kleiner, Ph.D.
Hilary Koprowski, M.D.
Gertrude Kornfeld, Ph.D.
Lawrence S. Kubie, M.D.
Marvin H. Kuizenga, Ph.D.
Raphael Kurzrok, M.D.
Willis E. Lamb, Jr., Ph.D.
Alexander D. Langmuir, M.D.
James Hain Leathem, Ph.D.
Philip Levine, M.D.
Howard B. Lewis, Ph.D.
Raymond L. Libby, Ph.D.
Urner Liddel, Ph.D.
John R. Loufbourow, Sc.D.
John K. Loosli, Ph.D.
Lawson G. Lowrey, M.D.
William H. McCastline, M.D.
Philip D. McMaster, M.D.
Thomas L. McMeekin, Ph.D.
Benjamin Malzberg, Ph.D.
Paul V. Manning, Ph.D.
Louis W. Maraventano, M.D.
Henry C. Marks, Ph.D.
Charles A. Marlies, Ph.D.
Joseph Mattiello, Ph.D.
Nicholas A. Milas, Ph.D.
John A. Moore, Ph.D.
Robert S. Morison, M.D.
John H. Mulholland, M.D.
Otto H. Muller, Sc.D.
Carl Neuberg, Ph.D.
Leslie F. Nims, Ph.D.
Friederich F. Nord, D.Sc.
Byron J. Oakes, Ph.D.
Fairfield Osborn
Gerald Oster, Ph.D.
Raymond C. Parker, Ph.D.
Arthur L. Patterson, Ph.D.
Ely Perlman, M.D.
Jean Piatt, Ph.D.
Zygmunt Anthony Piotrowski, Ph.D.
Bret Ratner, M.D.
Jonathan E. Rhoads, M.D.
Walter S. Root, Ph.D.
Harry M. Rose, Ph.D.
Antonio Rottino, M.D.
E. A. Rovenstine, M.D.
John B. Russell, M.S.
Amram Scheinfeld
Maxwell Schubert, Ph.D.
Jack Schultz, Ph.D.
Ervin Schwenk, D.Sc.
Morton D. Schweitzer, Ph.D.
Leo Shedlovsky, Ph.D.
Simon Shlaer, Ph.D.
Ephraim Shorr, M.D.
H. O. Singher, Ph.D.
Harry H. Sobotka, Ph.D.
David M. Spain, M.D.
Herman T. Spieth, Ph.D.
Henry B. Steinbach, Ph.D.
Y. SubbaRow, Ph.D.
Percival M. Symonds, Ph.D.
Maurice L. Tainter, M.D.
William Thalhimer, M.D.
Hans E. Thalmann, Ph.D.
Henry C. Thomas, Ph.D.
Max Tishler, Ph.D.
Richard Tislow, M.D.
John E. Vance, Ph.D.
Andries Voet, Ph.D.
Peter Vogel, M.D.
John Volkmann, Ph.D.
George Wald, Ph.D.
Roland Ward, Ph.D.
John C. Warner, Ph.D.
T. J. Webb, Ph.D.
Edward R. Weidlein, Sc.D.
John H. Welsh, Ph.D.
Mark D. Welsh, V.M.
E. Clifford Williams, D.Sc.
Leonard George Worley, Ph.D.
Marjorie B. Zucker, Ph.D.

Honorary Life Membership was conferred upon the following eminent scientists:

V. Bjerknes, Professor of Meteorology, Institut d'Astrophysique Théorique, Oslo, Norway.

Niels Bjerrum, Director, Royal Danish Agricultural College, Copenhagen, Denmark.

Niels Bohr, Director, Institute for Theoretical Physics, University of Copenhagen, Denmark.

Sir Henry Hallett Dale, formerly Director of National Institute for Medical Research, Mt. Vernon House, London, England; at present, Chairman of the Board of Trustees, Wellcome Physiological Research Laboratories.

Albert Defant, Director, Institute of Meteorology and Geophysics, Vienna, Austria.

ELECTION OF OFFICERS

The following officers were elected for the year 1948:

For President
Harden F. Taylor

For Chairmen of Sections
Henry D. Thompson
Clairette P. Armstrong
Serge A. Korff
Charles R. Schroeder
William Duncan Strong
Bernhard Haurwitz

For Recording Secretary
George B. Pegram

For Corresponding Secretary
Athelstan F. Spilhaus

For Treasurer
E. Clifford Williams

For Editor
Roy Waldo Miner

For Councilors (1948-1950)
Ross F. Nigrelli
Victor K. LaMer
Fredrick F. Yonkman

For Finance Committee
Addison Webb, *Chairman*
Charles N. Frey
John A. Zellers

PROGRAM OF THE EVENING

After the Business Meeting, the following papers were presented under the heading:

NEW YORK AS THE CROSSROADS IN THE WORLD OF SCIENCE

LEARNED SOCIETIES AND THE EDUCATIONAL ASPECTS OF FUTURE SCIENCE: THE NEW YORK ACADEMY, AND THE SYNTHESIS OF THE SCIENCES

By Harden F. Taylor
President, The New York Academy of Sciences

FACTORS SHAPING THE FUTURE OF INDUSTRIAL SCIENCE

By George A. Sloan
President, The Nutrition Foundation, Inc.

INTERNATIONAL ASPECTS OF SCIENCE

By W. Albert Noyes, Jr.
Professor of Physical Chemistry, University of Rochester, and President, American Chemical Society

These papers are published on the following pages of this number of the Transactions.

LEARNED SOCIETIES AND THE EDUCATIONAL ASPECTS OF FUTURE SCIENCE

The New York Academy, and the Synthesis of the Sciences

By HARDEN F. TAYLOR*

On February 24, 1817, a group of 21 scientists met at Harmony Hall, Duane and William Streets, New York, signed the constitution, and launched the Lyceum of Natural History on its career. The name was fitting; *Lyceum* reflected the classical education of the time and still prevailing veneration of Aristotle, while *Natural History*, embracing all the descriptive outdoor sciences, was the most advanced branch following Linnaeus, Buffon, Lamarck, and Cuvier. In 1876, the organization changed its name to The New York Academy of Sciences as being more correctly descriptive of its true nature and functions and the much changed course of science. There are only three scientific societies in America senior to The New York Academy of Sciences, *viz.*, The American Philosophical Society of Philadelphia, 1769, the American Academy of Arts and Sciences, Boston, 1780, and the Academy of Natural Sciences, Philadelphia, 1812.

The five counties that now constitute New York City then had a population of 140,000, about that of Yonkers today. The population of the whole United States was a trifle over 9 million, 90 per cent farmers, and considerably less than the population of the New York metropolitan area today, which is about 12 million.

Eight days after the initial meeting in Harmony Hall, James Monroe was to take over the presidency of the United States from James Madison; our war with Britain had ended two years earlier, the Napoleonic wars 20 months before at Waterloo, and the second Peace of Paris, fifteen months before, had settled the issues and launched the western world on the most remarkable and fruitful century in all history, a century marked by only a dozen or so local wars of short duration, and by the flowering of the scientific and industrial revolution which immeasurably elevated the standard of living in material things, enriched and extended the duration of individual life, and made it possible to support, nourish and employ vastly increased populations.

Life in February, 1817, was as yet untouched by the new things to come. There were no railroads; transportation and communication on land was by stagecoach and wagon over muddy roads, at sea by sail, requiring a month to six weeks for a passage to Europe, with salt beef, hard tack, beans and cereal for food on the way (first steam crossing, *Savannah,* March 28-June 20, 1819); there was no way of keeping perishable foods other than drying, salting, spicing, etc., or in cool springs and wells or window boxes in winter for a short time; hermetic canning, although invented in 1810, was not to arrive on this continent till 1840 (in U. S., 1843); all writing and copying was by hand with quills; there was no central heating of houses; the steam boiler was highly explosive and engines without ball governor and Corliss valves, both to follow in 20 to 30 years, were highly inefficient and troublesome; photography had not been thought of; rubber was to be a useless gum for another 22 years before vulcanization: steel making had to await the Bessemer and the open hearth processes for another 40 to 50 years; mechanical refrigeration was equally remote. The first dental college in the world was 25 years away in Baltimore: the practice of medicine was largely in the administration of galenicals, purges, and emetics; anesthesia was another 25 or 30 years in the future; electromagnetism, electromagnetic induction, and the laws of the electric current were yet to be discovered, as were the synthesis of organic compounds, the nature of heat and its mechanical equivalent, and of light, the periodic system of chemical elements, the cellular structure of plants and animals, asepsis, bacteria, phagocytosis, immunity, photosynthesis, the extension of astronomy beyond the solar system, and most of our other great scientific laws and generalizations.

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* President, The New York Academy of Sciences.

Although great things were already in the making, the men of science who were to carry out the remaking of the world were yet to come. Charles Darwin was a lad of 8, and his embarkation on the voyage of the *Beagle* was 14 years in the future; Claude Bernard was 4 years old; Bunsen and Schwann were 7, Louis Agassiz was 10 years old, Matthew Maury 11, Schleiden 13, Liebig 14, and Sir Charles Lyell and Joseph Henry were 20. The arrival of Joule on the scene was to be a year later, Helmholz and Virchow 4 years, Pasteur, Clausius and Gregor Mendel 5, Kelvin 7, Cannizzaro 9, Clerk Maxwell 14, Mendeleev 17, Archibald Geikie 18, Perkin 21, and Willard Gibbs 22.

DuPont's powder factory was working down on Brandywine Creek near Wilmington, but no industrial enterprise had ever given a thought to employing a scientist in order to improve its business. Most of the scientists were men of leisure, some were clergymen, university lecturers, directors of botanical gardens, and a few were busy people such as Franklin had been, who satisfied intellectual curiosity with science as a hobby.

We might almost say, without too much exaggeration, that in 1817 one man with an exceptional I.Q., a highly retentive memory, and willingness to study diligently could know all there was to be known of science of that day.

Such was life in 1817.

I do not need to retell the story of the accomplishments of science during the ensuing century until we entered World War I, in 1917. We select for particular attention for our purposes here the natural and necessary specialization of sciences into more and more and smaller and smaller compartments as knowledge grew, data accumulated and their relationships became more complex.

During that century of progress, many, if not most, of the great accomplishments were achieved each within the framework of its own specialty—the electrical, mechanical, thermodynamic, chemical (both organic and inorganic), and metallurgical achievements were mostly within their own spheres of science.

However, in the latter part of the period, and to a rapidly growing extent, the need has been apparent for more synthesis in the solution of great scientific problems, *i.e.*, bringing to bear on them the knowledge and techniques of two or more and sometimes many of the specialized branches of science. An example was the establishment of physical chemistry.

The multiplication, proliferation, and expansion of scientific effort, the number of men engaged in it, the elaborateness of organization, the expenditure of funds for support, the multiplicity of specialties and their ramification into every remote corner of human interest have now proceeded so far that no man can even conceive or be aware of more than a small part of the total. A few days ago, the list of the scientific publications abstracted in *Biological Abstracts* was sent to scientists in the biological field. It contained 2,313 titles of publications regularly abstracted. The American Chemical Society regularly abstracts about 4,300 publications. If we allow liberally for duplication and for publications enumerated which are only on the fringe of science, we may estimate the number of scientific publications abstracted by the two organizations as 5,000. The two November issues of *Chemical Abstracts* (which appears twice a month) had 519 pages with an average of 10 abstracts per page, or over 5,100 papers for the month, or 61,000 papers per year, and in several languages. In the two November issues of *Chemical Abstracts,* a total of 7,275 authors and co-authors are indexed.

The extremity to which specialization has gone may be illustrated by a review written by J. T. Randall of Maurice Curie's book, *"Fluorescence and Phosphorescence"*, in *Nature* for October 11, 1947. Says Randall:

> "The luminescence of vapors, of solutions, of solids, and the phenomena associated with chemi- and bioluminescence all fall within the scope of the subject. Because each of these divisions requires, at any rate to some degree, a different type of background and objective, it is unusual for any one person to apprehend the whole field."

During the later years of our scientific century, it began to appear that many of the major problems awaiting solution had not yielded to attack by specialists (though some of the sciences, such as oceanography, medicine, and geology, were composite sciences all along), and that a concerted attack would be required by convening the efforts of numerous and diversified specialists. The cancer problem is one of these. Numerous others were solved by such organized and concerted effort during the late war. A great many more are and will be before us; indeed, it appears that from now on, more and more of the great problems to be solved will be of the kind which are solved only by concerted effort.

In keeping with the growing need for synthesis of the many sciences in dealing with present problems, the Academy inaugurated a few years ago the Conferences on chosen subjects. These conferences, usually running two days, are designed to bring together from wherever they are the leading researchers on a particular problem, often dealing with it with widely different techniques and from the points of view of different branches of science. The participants come from all over the country and from foreign countries, wherever they may be. The results are usually published as a separate Article of the *Annals.* These conferences have had a gratifying degree of success and are to be continued and increased in number as the Academy's resources permit. They have been, so far, in the fields of medicine, oceanography and meteorology, physiology, nutrition, synthetic organic chemistry, parasitology, pathology of lower animals, psychology, chromatography, animal behavior, physics, and astronomy.

The New York Academy of Sciences is peculiarly well situated geographically and by tradition to serve all the sciences in this way. It is situated in the center of what seems the largest and most concentrated area of scientific activity to be found in the world. If we describe a circle with radius of 50, 100 or 200 miles from where we now are, the circle would contain probably more scientists, and more scientific research and activity, than any other similar area. If we define the area as that accessible overnight by train or two hours by airplane, the cities of Boston, Providence, New Haven, Hartford, Albany, Troy, Schenectady, Syracuse, Philadelphia, Pittsburg, Cleveland, Akron, Buffalo, Rochester, Binghamton, Wilmington, Washington, Baltimore and Trenton, and many other smaller cities, would be included. Within a radius of a hundred miles are colleges and universities of world-wide importance—Columbia, New York, C.C.N.Y., Rutgers, Yale, Vassar, Princeton, Pennsylvania, Fordham, Stevens and Brooklyn Polytechnic, and slightly further away are Pennsylvania State, Brown, Cornell, Syracuse, Amherst, Williams, Connecticut, Harvard, Rochester, Bryn Mawr, Wellesley, Smith, Lehigh and Rensselaer, and many smaller colleges and technical schools. Within the area are the largest number of hospitals, medical and dental centers, and colleges and schools of pharmacy; research institutions—Rockefeller, Merck, Squibb and Warner Institutes of Medical Research, Boyce Thompson Institute of Plant Research, Institute for Advanced Studies at Princeton, Wistar Institute, Nutrition Foundation, Woods Hole Oceanographic Institution, Marine Biological Laboratory, The American Museum of Natural History, New York Zoological Society, the State Departments of Agriculture, Public Health and Social Welfare of New York, New Jersey and Connecticut—indeed, as we look over the list of such institutions and foundations, it appears impossible to mention all or even the most important ones. We are equally helpless in the mention of industrial corporations who maintain research departments, having such names as Air Reduction, Allied Chemical & Dye, American Cyanamid, American Locomotive, Bakelite, Bell Laboratories, Bendix, Celanese, Ciba, Colgate-Palmolive-Peet, Edison, Fleischmann, General Aniline & Film, General Electric, General Foods, H. Heyden, Hoffman-La Roche, Johnson & Johnson, Metal & Thermit, S. B. Penick, Pfizer, M. W. Kellogg, Roebling, Schenley, Schering, Schieffelin, Socony-Vacuum, Squibb, Standard Oil of New Jersey, Sterling-Winthrop, Union Carbide, U. S. Rubber, Warner, Wellcome Laboratories, Westinghouse Lamp, and literally dozens of others; slightly further away the names of Atlas Powder, Bausch & Lomb, Du Pont, Distillations Products, Eastman Kodak, Hercules Powder, Hooker Electro-Chemical, Leeds & Northrup, R.C.A. Situated in and around New York are also many consulting and service chemical, bacteriological and engineering laboratories, as well as numerous endowed foundations for research in particular scientific fields. New York is also the seat of headquarters for important branches of numerous scientific societies, such as the American Cancer, Geographical, Mathematical and Physical Societies; the Archaeological, Illuminating Engineering, Microchemical, Chemical Industry, Geological Societies; American Immunologists, Industrial Physicians and Surgeons, Orthopsychiatric, Psychiatric, Public Health and Standards Associations; Associations of Child Study, Modern Languages; American Institutes of Mining Metallurgical Engineers, Chemists, Chemical Engineers, Physics, Electrical Engineers; the Institute of Aeronautical Sciences; and many others.

In the midst of this scientific galaxy, the Academy is not only the fourth oldest in the country with its roots in the origins of our scientific era and an unbroken record since: it is now the only scientific society which serves the whole body of science in all its branches here. It has six sections and nine affiliated societies with regular meeting times and places and publishes the *Annals* which began in 1824 and *Transactions* in 1881. For its purposes, it makes no important distinction among the pure or academic, the professional service and the industrial applications of science; they all add to our sum of knowledge and understanding of nature and promote the welfare of mankind.

Throughout its long career, the Academy's size, influence and fortunes have waxed and waned with circumstances of the times. Most of the time it has needed a home; it acquired one in 1835 but lost it by foreclosure of mortgage in the depression of 1844 after a period of inflation somewhat such as we are now having, and it lost its natural history collection in a fire in 1866.

In 1902, the Academy accepted an invitation to make its permanent home at the American Museum of Natural History, including its library which had been moved around from pillar to post for 85 years. In 1926, the library was sold to the Museum.

The 45 years in the Museum have been a happy period, but the arrangement is now no longer suitable. The Academy has outgrown its home in both size and activity. In April of this year, the Council voted reluctantly, but of absolute necessity, to limit the membership for the time being to a total of 4,000 members in the Student and Annual Active Classes. On December 6th, its membership of all classes was 4,009, which is more than ten times the membership in 1935. This membership, representing all 48 states, the District of Columbia, two territories and 28 upon a foundation of scientific data and natural laws, making use of the great mass of knowledge accumulated throughout the centuries. Prior to the rise of industrial research, scientific or fundamental research was centered largely in schools, universities and other institutions of learning.

All of the teachings of basic science have, in due time, influenced the course of industrial research, and the principles flowing from the work conducted in university laboratories have served to make possible a large proportion of our industrial development. Thus there has come to be, many believe, an over-emphasis on the achievements of industrial research. Too infrequently do we pause to consider the extent to which applied science has drawn upon so-called pure science for support.

The atom bomb, for instance, which today holds the public imagination, is popularly believed to have been developed solely by intensive research prosecuted under great pressure during the second World War. True enough, it emerged as a device or weapon during that period, but back of the intensive phase of wartime development lay approximately 50 years of patient scientific study and research concerned with atomic structure, radioactivity and energy-mass relationships, all of which combined to chart the course for the eventual achievement Today, the opportunity exists for scientists to proceed again, perhaps for many more patient years, to discover new knowledge which will, we may reasonably hope, harness nuclear fission to more productive uses than war That is the penalty and the reward of research—every discovery opens many new avenues for continued investigation and study.

Night after night, most of us are subjected to the flashings of multitudes of neon signs. We are apt to consider them as the discovery of some smart advertising man. We fail to realize that this commercial device is merely the adaptation of scientific studies having to do with the nature of electrical conduction in gases. Had it not been for the basic study of the pressure, volume, and temperature relationships of gases, there would be no neon gas available for exploitation.

The modern vacuum tube is a marvelous device, performing all sorts of services for mankind, contributing in a thousand ways to industrial and social progress. It was perfected by painstaking scientific work in industrial laboratories, but its roots go back to fundamental investigations which were not directed primarily toward commercial application.

Similarly, our modern systems of mechanical refrigeration owe a great debt to the study of vapor pressure and the long and careful development of the laws of thermodynamics which long preceded the clever adaptations that, in due time, translated these matters into everyday servants.

At almost every turn, we find that basic science has provided a well-laid foundation upon which applied science has erected outstanding implements of the modern world. Many of our finest corporations owe their present position of leadership to a far-sighted policy toward scientific research. We have now reached the fortunate stage where we can cite a steadily growing trend to include research in company budgets. This helps to bring the personnel of industrial research groups very close to the fields being explored in our strongest university centers.

The benefit of such a liaison redound upon both groups. Today, however, with the great emphasis placed on research, there is considerable confusion in the minds of the public and of the politicians as to the proper function of research, and how it should be controlled. Indeed, here lies the most dangerous pitfall in the entire picture—the risk that out of an intended measure to develop and strengthen research, political control may gain a disastrous foothold inside the laboratories where men are and should be simply and sincerely seeking after the truth. It would be better for future generations if the spirit of scientific endeavor could be introduced more widely into political life rather than the reverse.

This situation has not been simplified by the fact that numerous universities are now reaching out into the fields of applied, commercial research, while a number of large industrial laboratories are including a considerable amount of basic scientific investigation in their programs. Meanwhile, the Government is providing increased laboratory facilities for research work on ,problems of interest both to national defense and public economy, and proposals come before Congress for a tremendous enlargement of Government research activity.

There are many honest differences of opinion as to what kind of research should be continued by universities, Government and industry. Universities, faced with depleted funds, believe they have a logical source of income by commercializing applied research projects. This risk of diverting university research efforts from the more fundamental types of work should be guarded against. The Government, confronted with problems in national defense, has many advisers who sincerely believe it is the duty of the Government to protect the national interest by financing a large program in Government research laboratories. Industry is inclined to work as independently as possible. But this does not imply any desire or need to avoid the fullest cooperation with either university or Government research groups.

In general, all practical applications stem from the progress which is made in fundamental science. If this stream should dry up, the progress of practical application would continue, but the scope would fail to enlarge as it has so consistently done in the past. Hence, it is of vital importance that the universities continue to foster fundamental scientific investigation, because it is only in such an atmosphere that we can rely upon continued progress. It is also to these sources that we must look for the thorough training of the scientists to supply industry with their working forces.

It is important that the search for knowledge be unfettered and untrammeled. Ideas are not made to order nor in accordance with pre-determined schedules. Neither are they nourished by political pressures or by a desire to serve a party interest. If we turn over to Government the major part of our future efforts for the advancement of science, then we deny the effectiveness of our system of free enterprise; we subject science to illogical pressures, and we risk the stultifying effects which can come with too much regimented, Government control.

Industrial research organizations will continue to engage in fundamental studies, usually directed toward the development of new knowledge in fields in which their industries are particularly interested. This is right and proper, and experience has shown it to be useful and valuable. More and more of this kind of work will be done, but it can never be enough. The blank spaces in the pages of knowledge are too vast. Yet, I believe, industry today completely recognizes the need for all of the university types of scientific research that can be provided—the types that provide the building blocks from which many technical adaptations can be made.

Industry knows that the greatest yields of basic science have come from research inspired by a desire to learn the unbiased truth and to roll back the boundaries of ignorance. In times past, our institutions of higher learning have provided an atmosphere in which fundamental scientific research could grow without prejudice. It is the part of wisdom to maintain that favorable atmosphere. Industry more and more will come to recognize its share of responsibility for supporting and maintaining institutions of higher learning.

My faith in that direction arises in no small part from experience with the

Nutrition Foundation. This Foundation was created in 1942 by some of the leaders in the food industry, because they recognized the public responsibility of their industry. They saw, too, that there was a pressing and permanent need for fundamental research. They knew that the food industry was essential to high national production and a high level of national health. They recognized, too, that there was need for a wide degree of freedom in research. They realized that research controlled by specific objectives, or research too closely related to immediate return, would be so limited as to fall far short of the goal.

Thus, the Nutrition Foundation came into being. Its purpose was to carry out a program which was truly scientific, but not to undertake research within its own laboratories. Its method was to finance approved projects in universities and medical centers. Doctor Charles Glen King of the University of Pittsburgh was selected to direct the scientific program. He is aided by an eminent Scientific Advisory Committee, composed of 15 of the most active and critical research leaders in the United States and Canada. This Committee reviews all applications for grants-in-aid and approves all projects. It is this committee which decides what areas of research are likely to bring the most important results, and where the urgency of support exists. In so doing, they accept a responsibility to the science of nutrition as a whole.

The program has had five main long-range objectives, namely:

(1) Research directed toward discovering and measuring the human nutrient requirements.

(2) Research to find how the various nutrients function inside the body and how each can be measured accurately.

(3) Research dealing with maternal and infant nutrition.

(4) Research in which there is direct emphasis upon relationships between nutrition and health.

(5) Education, beyond that provided by research grants, to facilitate the application of new and sound information.

The Foundation has been in operation some six years, which, in the field of science, is hardly more than momentary. Yet it has achieved significant results and contributed to the increased knowledge of nutrition. Let me enumerate a few of the results which have come from Foundation grants:

The determination of adult human protein requirement in terms of essential amino acids.

Appraisal of a new vitamin of the B group important in protecting mankind against anemia.

A more complete knowledge of human milk and cow's milk.

A clearer insight regarding the role of certain hormones in health and disease.

These and many other contributions have been made to the basic science of nutrition and to the well-being of man. How much have they contributed to the applied science of the food industry?

Frankly, I cannot tell. Our responsibility is to basic science. The application of this basic science to the food industry is primarily the responsibility of the scientists in industrial laboratories, and that, I believe, is the way it should be. The Nutrition Foundation, supported by an increasing number of food companies, large and small, applies itself to basic research and publishes the results of its work for those able to put these findings into practical application in whatever area they may be —medicine, education, public health, home economics, or the food industry.

This, I believe, offers a pattern for many other industries to follow. Indeed, it is now being followed in two notable instances. In 1944, the life insurance companies of the United States organized the Life Insurance Medical Research Fund. In January of this year, announcement was made of a new foundation organized by the Pharmaceutical Industry. Both of these organizations are patterned along similar lines to the Nutrition Foundation, in that funds are provided for basic research grants to universities and medical institutions, and based upon the recommendations of a distinguished Scientific Advisory Committee. Industry knows basic science is best nurtured in universities and institutions of learning, and it will accept responsibility for supporting basic research for the development of both scientists and knowledge. Its support of scientific investigation will not, in my opinion, be closely restricted to its own direct interests. Industry knows that advances in any field of science may well bring it a return. Only recently one of the large oil concerns made a very sizable contribution to a project for broad medical research. That contribution will probably never bring a penny return in increased sales of oil, but it may well bring great returns in the health and happiness of the company's employees and the sound health of the community in which it operates.

I venture to predict that industry will give increasingly of its resources to support the broad cause of higher education. Industry knows that it must look to our universities and colleges for its source of trained and educated manpower. If our standards of higher education are permitted to deteriorate, then the standards of manpower will decline. The problem of financing higher education is today a serious one for hundreds of our independent institutions.

Should the task of the universities be turned over completely to state or federal institutions? I believe not. I believe the American public will see to it that private funds keep our system of higher education reasonably independent of political control. And industry, accepting a broad concept of citizenship, can play an important part in this accomplishment.

INTERNATIONAL ASPECTS OF SCIENCE

By W. ALBERT NOYES, JR.*

Science has become a matter of political as well as economical and social importance, due to the fact that it is essential for the prosecution of modern war. Scientists themselves have not fully recognized the important position that they occupy in the world today, although a minority realize that they have contributed to making war more awful and, through a sense of guilt, have come to believe that it is high time they took a greater interest in the cause of peace.

In their own international relationships, scientists have a common ground which may give them a delusion of having the clue to a peaceful solution of world problems. Even when they do not speak the same language, they often understand each other's symbols and can understand each other's words. It is remarkable how much information one can often obtain from a scientific article written in a language one has never studied. When discussions at scientific congresses leave the purer scientific subjects, one usually finds that scientists have the same weaknesses as other individuals. In the period between the two World Wars, the isolationism of this country was reflected in an apathy toward international scientific organizations, and in most of them this country played a relatively minor role. In attempting international collaboration in matters of scientific publication, national pride prevented working agreements just as effectively as it did in the broad field of international politics.

The situation since the close of World War II has been somewhat better, and there are signs that the scientists of this country have begun to realize that, as representatives of a great nation, they are expected to show some leadership in international scientific organizations. A long history of ineffectual participation makes the progress slow, and it is only fair to state that scientists of other countries are afraid of United States domination just as many other countries fear the domination of the United States in military and political spheres. There are more chemists in the United States than in all of western Europe, and for that reason it is easier to finance publication programs in this country than it is abroad. We often feel that it is more convenient, as well as more efficient, to carry on by ourselves than it is to elicit the cooperation and support of other nations. All too often we forget that actions which appear to us to be for the good of science appear to others to be nationalistically selfish.

International organizations in science have existed for more than 100 years, although they became numerous and effective only after World War I. These organizations have generally had poor financial support, but they have served useful functions in obtaining international agreements on some of the fundamental constants of nature, the atomic weights, symbols and nomenclature. Participation by this country in the various international scientific unions was hampered by lack of funds. Delegates to meetings were chosen mainly among those who happened to be traveling to Europe as tourists, or among a small group of persons financially able to pay their own expenses. This situation still needs rectification, although attempts are being made to develop an informed scientific opinion in this country so that delegates in the future will have some means of knowing the official attitude of American scientists toward the various problems being discussed in these international meetings.

The international organizations which existed before the war were mainly effective among those nations where science was relatively strong, that is, in western Europe and in North America. Other parts of the world had too few scientists and too weak national technical organizations to participate effectively. But the role of science in preserving peace must be viewed in a broad light, and hence the various United Nations organizations deserve our attention and support. Several of these deal with scientific problems, such as health, food, agriculture, meteorology, and aviation, but the United Nations Educational, Scientific, and Cultural Organization is the one which deals mainly with pure science. This organization was established formally in Paris during the fall of 1946, although a Preparatory Commission had been surveying the field and developing a program for many months previously. We have not time to give many details of the UNESCO program in science. Since UNESCO covers many disciplines, a certain rivalry has existed, and the attitude of the United States has apparently been one of fear that science would dominate the organization. To some extent, this fear may be justified because scientists are acquainted with each other, familiar with each other's work, and usually have little difficulty in arriving at a program which is very useful and generally accepted. Other parts of UNESCO, such as the ones dealing with education, the humanities, and the social sciences all border on political subjects wherein differences in ideology create problems of a real nature in formulating a program. Thus, it is probably no accident that the section on Natural Sciences of UNESCO has made more real progress than most of the others.

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* Professor of Physical Chemistry, University of Rochester, and President, American Chemical Society.

The organizations established after the close of World War I were ineffective largely because they were innocuous. They made scholarly studies in many important domains, but through lack of funds and perhaps, also, through fear of intruding in national fields, they did not become operating organizations. There is a similar tendency evident in the thinking of many persons about UNESCO. Since the problems of the Science Division are relatively concrete, and there seems to be fairly general agreement that UNESCO must do more than make harmless studies, the Science Division has naturally asked for a larger share of the budget than some others are willing to grant. Moreover, it seems to be the philosophy of certain individuals, including several eminent persons in the United States, that every project should be viewed in the light of its effect on the cause of peace. International scientific congresses, exchanges of students and professors, publication of scientific journals, and rehabilitation of scientific laboratories are all worth while in themselves, but one cannot say that a program of that type would ensure peace either in the near or in the long range future. This attitude has given the scientific part of UNESCO's program some real obstacles to overcome, but fortunately, at least for the scientists, other nations give science a more prominent position than does the United States.

Organizations of scientists and contacts between scientists are important for the cause of science. It is essential to have a wide dissemination of scientific information so that techniques, new theories, and experimental facts may be made available to any scientist who can use them. Military security stands squarely in the way of much progress today, although certain restrictions appear to be necessary until more satisfactory agreements have been made in the political sphere. Nevertheless, the impact of science on social and economic problems must be considered. The decision for or against war now lies in the hands of statesmen and will be little influenced by anything a scholar can do in the immediate future.

One sees in the world, today, various forms of government. In this country, we profess to believe in free enterprise, not realizing fully that free enterprise in the broadest sense has become obsolete. When a large fraction of a population works for big corporations or for the government, individual freedom is, to a large extent, circumscribed, and one is dependent on forces beyond any single person's control for gainful employment. Since the standard of living depends upon rate of production, it is evident that any economic system which causes widespread unemployment has failed to maintain a standard of living as high as possible. In countries with dense populations and with insufficient raw materials, a high standard of living is impossible unless the population is kept busy making things for export which can in turn be used to pay for importation of foodstuffs. Science may aid materially in increasing rate of production in overpopulated countries, but there are countries today which could not possibly attain a standard of living comparable to that which we have in the United States. What products can many of these countries make more economically than Americans can make them? In other words, what could we buy abroad in exchange for the food which other people are forced to receive from us? If raw materials are more plentiful here, and mass production methods have been developed to a higher degree, it seems inevitable that it will take fewer man hours to produce a given quantity of goods here than elsewhere. Aside from a few luxury items, what kinds of goods could we import from western Europe to pay for the foodstuffs we are sending over?

Unless a solution to this problem is found, western Europe will have to be treated as a charity case, and no decrease in tariff barriers or other removals of restraints on trade will provide an answer. Thus, one sees that scientists will be unable to provide anything like a uniform standard of living throughout the world. Unless the standard of living is more or less uniform, there will always be causes for jealousy, friction, and war. Large populations of the world are not going to be content to live in the international poorhouse. The difficulties we encounter are so intimately mixed up with religion, economics, and politics that the scientist must recognize that he alone cannot solve the problem of peace.

Some important steps, however, are possible. Certain countries, including some in western Europe, train only a handful of scientists because the whole system of education is not based on a democratic principle. We have learned one lesson thoroughly in the United States, namely, that for every scientist with new and brilliant ideas, scores and hundreds of less competent but scientifically trained men are essential to do the everyday work and to put new ideas into public use. One Nobel Prize winner, in other words, does not make a country strong. Hence, an important thing for UNESCO or for any other international scientific organization to do, is to broaden the base of education so that each country will be adequately supplied not only with pure scientists but with applied scientists and engineers. Even in countries devoid of great natural resources, much can be done to improve sanitation, provide better housing, and utilize such power sources as do exist. Perhaps, eventually, atomic energy will provide a source of power for those countries lacking coal, oil and water power and unable to export. Sources of power are necessary for a high standard of living.

The program of UNESCO takes into account the need of raising the scientific level throughout the world. Regional Field Cooperation Stations are being established which will aid in the interchange of scientific information. A project in the Amazon Basin may combine research on the utilization of tropical resources with a program of higher education.

We must not, however, confine our attention to applied science. Pure science has its cultural value just as do literature, art, and music. The more we can provide a common basis for culture throughout the world, the better chance we have for mutual understanding and confidence. Science has few equals among the other disciplines in this respect except for one difficulty—the small number of persons who really understand it. Therefore, we make a plea for internationalism in science, not only for the material things it can do in reducing friction by raising the standard of living, but in providing a culture which could be common to all people.

Abstract: ENERGY SOURCES IN GIANT STARS

By DONALD H. MENZEL*

Awarded an A. Cressy Morrison Prize, 1947

The rate of energy generation for the red giants, based on Bethe's carbon cycle and the standard equilibrium models of stellar constitution, is far less than that observed. The alternatives are to abandon either the carbon cycle or the standard model. I have chosen the latter and propose, as a general model for giant stars, one whose atmosphere is greatly distended by dynamical motions. It may even consist of an effective shell, partially detached from the stellar nucleus. The giant stars are thus represented as a sort of compressed planetary nebula, or as a Wolf-Rayet star whose envelope is appreciably denser than the average W-star.

Evidence supporting the validity of this picture includes, especially, Merrill's "symbiotic" stars, like R Aquarii, which simultaneously shows characteristics of a low-temperature long-period variable, a hot star, and a planetary nebula. In this paper, we identify the hot star with the nucleus, the red star with radiations from a partial shell, and the nebula with an external gaseous envelope excited by ultraviolet radiation from the nucleus.

The hypothesis enables one to relate almost the entire range of variable stars, from the Cepheids, through the long-period variables, on to the catastrophic variables.

The paper discusses, in some detail, various theories of atmospheric excitation and support. It is concluded that Kiepenheuer's theory of slowly varying magnetic fields in spots is inadequate to account for the excitation of the corona. However, if there are rapid magnetic variations of small amplitude, the heating effect will take place, as a sort of electrode-less discharge. Such variations, however, will produce radio waves of very low (audio) frequency. Recent experiments by Salisbury and myself have shown that electromagnetic pulsations of this order actually exist at the surface of the earth, though their solar origin has not yet been established.

If such waves do exist, they are a mechanism for cooling a spot. They may well be important for many problems of stellar variation, if stars have spots like the sun. Babcock's recent observations of intense general fields of the stars favor the idea that stellar electromagnetic processes may be fundamental.

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* Harvard Observatory, Cambridge, Mass.

Abstract: THE EFFECT OF *CLOSTRIDIUM TETANI* TOXOID IN THE RELAPSE OF MALARIA

By EUSEBIO Y. GARCIA*

Awarded an A. Cressy Morrison Prize, 1947

Over 300 patients, naturally infected with *Plasmodium vivax* and/or *P. falciparum,* were classified with reference to (1) primary or secondary infections, and (2) residence in endemic or non-endemic regions. All received basic treatment consisting of one intramuscular injection of 0.1 gm. of atebrine, followed by either (1) a total of 2.1 gm. of atebrine tablets taken by mouth at five-hour intervals for 7 days, or (2) a total of 3.5 gm. of chloroquine tablets by mouth for the same interval and duration. This treatment removed almost 99 per cent of the asexual stages from the peripheral blood in 20 hours and both asexual and sexual stages at the end of 7 days. This treatment was followed by relapses in 90 per cent of *P. vivax* and 83 per cent of *P. falciparum* patients at an average interval of 115 days.

If, however, 1 cc. of tetanus toxoid is injected subcutaneously at the end of the basic treatment and the same dose is repeated at the end of 3 weeks, none of the *P. falciparum* patients suffered relapses during the subsequent 315 days.

Of the *P. vivax* patients from endemic regions who received atebrine therapy and toxoid injections, 3.35 per cent of those with primary infections and 2.5 per cent of those with secondary infections suffered relapses in the ensuing 315 days.

Of the *P. vivax* patients from endemic regions who received chloroquine medication and toxoid injections, 16.6 per cent of those with primary infections and 6.6 per cent of those with secondary infections suffered relapses in the ensuing 315 days.

Of the *P. vivax* patients from non-endemic regions who received atebrine therapy and toxoid injections, 10.5 per cent of those with primary infections and 6 per cent of those with secondary infections suffered relapses in the ensuing 315 days.

Of the *P. vivax* patients from non-endemic regions, who received chloroquine medication and toxoid injections, 14.3 per cent of those with primary and 16.6 per cent of those with secondary infections suffered relapses in the ensuing 315 days.

These data suggest that patients from endemic areas, as well as those with recurrent infections, have greater resistance and suffer fewer relapses than those who have had less contact with the malarial parasites. Since tetanus toxoid is not specific for malaria, it appears that it exerts its influence against malarial relapses by stimulating and enhancing the immune mechanisms of the body. These mechanisms are cellular in origin and the hypothesis is in accord with accepted immunological theory.

These results probably represent the first successful attempts to inhibit multiplication of protozoan parasites by stimulation of immune mechanisms through a bacterial product.

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*Parasitologist and Senior Malariologist, Medical Research Clinic, Binan, Laguna, Republic of the Philippines. Abstracted by Professor Horace W. Stunkard, New York University, University Heights, New York, N. Y. The full paper will be published as ANNALS, Volume 50, Article 2.

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SECTION OF GEOLOGY AND MINERALOGY

THE SCIENCE OF SNOW AND AVALANCHES

By PAUL NIGGLI*

The petrologist distinguishes three main types of rock, according to their manner of formation. They are known as igneous, sedimentary, and metamorphic, respectively. Important clues as to the origin of these fundamental units of the lithosphere may be gained by the study of processes which are directly observable. Thus, in volcanic areas, we can study the phenomena that accompany the formation of rocks from the molten state; the deposition of sediments can be followed on the seashore and on the mainland; while processes of weathering and coal formation give an understanding of metamorphism.

In intercontinental mountainous countries like Switzerland, observations on *ice* and *snow* offer new possibilities for gaining insight into processes of petrological importance. Such processes are comparatively easy to understand, as only one substance is involved. The formation and vanishing, the deposition and transport of this crystalline solid can be studied during the course of each year, and, on account of their importance for water supply, snowdrifts and avalanches deserve the most careful attention.

The formation of ice coating on a water surface shows that a liquid or melt can, on cooling, solidify to a compact crystal aggregate. Taken in connection with observations made in volcanic areas in which molten magma from the interior of the earth consolidates to form rocks, we are left with no doubt that crystallization from melts is an important factor in rock formation. In particular, the study of the ice cover shows clearly that, when undercooling takes place and sudden crystallization simultaneously sets in around many nuclei, the individual crystal is unable to freely develop its characteristic shape, but may neverthelesss assume a definite orientation in respect to the surface of cooling. Taking into account the peculiar properties of water, we learn to appreciate the importance of the rate of crystallization, the accompanying change in volume, the temperature gradient, the amount of melt, and the nature of the convection currents.

A far more vivid insight into the processes of crystallization is provided by falling snow. It permits us to study the growth of crystal nuclei or groups of nuclei to form snow stars and snow flakes and the myriad-fold repetition of a process remaining constantly the same in principle, though varying in detail. The variability of the observed forms seems unlimited and it is only by concentrating on essential

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* Zurich, Switzerland. This lecture, illustrated by lantern slides and motion pictures, was presented at the meeting of the Section on January 5, 1948.

This number contains the announcement of the A. Cressy Morrison and George Frederick Kunz prizes for 1948 (p. 159).

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features and applying statistical methods that the existence of certain common traits in development can be detected. In this respect, we are dealing with quite a characteristic problem of natural science, namely, that of bringing individual and generally typical properties into line with one another.

This particular case is concerned with a crystallization out of water vapor and water drops. Depending on the circumstances of formation and the prevailing atmospheric conditions, the following products can be distinguished:

(1) Preponderantly acicular or prismatic crystals, single or in aggregates.

(2) Preponderantly tabular crystals with the dominant plane perpendicular to the elongated axis of the prismatic crystals. Such crystals, two-dimensional for all practical purposes, often possess a thickness of about one thousandth of a millimeter and very often show dendritic star-shaped forms.

(3) Complex aggregates in which both prismatic and tabular crystals participate.

(4) Rounded crystals or crystal aggregates and products of quite irregular shape.

If, as happens very frequently, crystallization out of the water vapor of the atmosphere sets in suddenly as a result of undercooling, chains of molecules rapidly attach themselves to each center of crystallization according to the chief bonds and parallel to the six symmetrical directions in the basal nets of the ice structure. The points that are formed attract further molecules and growth takes place in an outward direction. The inner parts are frequently not completely filled in, as the points prevent sufficient material from reaching them.

Thus, six chief branches at angles of 60 degrees to each other shoot out from the center to build a dendritic snow star. Side branches of the first order can grow out of these chief branches only when the points of the latter have moved sufficiently far apart to allow diffusion out of the surrounding medium to set in and the critical point of supersaturation to be exceeded. Side branches are, therefore, lacking near the center. Their opportunity to grow is limited, as they extend into the same space as the side branches of neighboring chief branches. The profusion of forms observed is the result of irregular diffusion and mutual interference. The slightest change in supersaturation and temperature produces alterations within the limits laid down by the structure of the patterns capable of growing and being preserved. Falling snow in calm weather thus is a crystalline precipitate on a large scale. Gravity in conjunction with the size and shape of the crystals determines the rate of fall, which may vary from a few centimeters to over two meters per second. The process of sinking is, therefore, comparable to the deposition of sedimentation of salts out of evaporating sea or salt water. Just as deposits of gypsum or rock salt collect at the bottom of standing water, so the accumulation of falling snow on the uneven surface of the earth produces a snow cover which no less than the examples mentioned is a crystalline aggregate, a rock.

The fabric to which these variously shaped crystals give rise is, at first, felted, powdery and very loose, with poor internal cohesion. When it has piled up on a steep slope, it can slip down, much as a heap of feathers might do. Such avalanches are called powder, wild snow, or new snow avalanches. More than 90 per cent of the volume of newly fallen snow is interstitial space filled with water-saturated air. The snow rock possesses an enormous so-called absolute porosity and a low bulk specific gravity. One cubic meter of powder snow formed at low temperature and under calm conditions weighs only 50 to 100 kg. Wild snow is even lighter, weighing but 25 to 50 kg. Certain modifications of these data frequently occur, for instance when, as so often happens in the mountains, wind or snow storms tend to shake down the snow, to split up the branches of the stars, and thereby effect a tighter packing. By storm and drift, grains become wedged between other grains and jam themselves tight. Under conditions of snow-drifting, the powder snow is replaced by wind-packed snow weighing 100 to 500 kg. per cubic meter. That also leads to a consolidation similar to the hardening of sand under wind action. Moist wind may also produce a sort of firnification by condensation of water vapor between the grains So-called wind slabs may ensue, or surface features such as cornices develop on the leeward and also on the windward side of ridges and slopes. Some of the forms of the wind slabs may resemble those observed among sand-dunes and in sand deserts, and are a counterpart to these aeolian sediments.

The sciences of glaciology and avalanches, with snow as the fundamental material, were established in the 19th century in the alpine countries, especially in Switzerland. One of the leaders in this classical period was Louis Agassiz, later a distinguished scientist in the United States. Another was the forester, Coaz. Curiously enough, mineralogists and petrologists have until recently given little attention to the snow rock as such, that is, to the stratified mass resulting from different snow falls in a winter season. During the last ten years, great advances in the science of snow and avalanches have been made in the United States, in Canada, Austria, Germany, Scandinavia and Russia, and also in Switzerland. Paulcke and Seligman were pioneers in studying the dangers of avalanches for the skiers in the Alps. An international committee with Prof. Church at its head was founded. We have a classical example of fruitful cooperation between pure and applied science in the fact that it was, at least in my country, the formation of avalanches, one of the greatest dangers of the high mountains, which provided the initiative for a closer study of the winter snow cover. Much scientifically valuable information has already been gained by observations on this seasonal rock which is formed, undergoes alteration, and disappears.

The layman's conception of mineral aggregates as being everlasting and unalterable proved to be untenable even in the case of silicate, carbonate. and salt rocks. It has even less justification in connection with snow deposits. It was one of the chief tasks of the observatories in the high mountains to unite theory, experiment, and observation of nature in studying the properties and alterations of the snow cover during the winter season.

The looseness of the aggregates and their low melting-point called for specially adapted methods of investigation which, for the most part, had to be developed first. It is, of course, impossible to collect hand specimens of the snow deposit and remove them for investigation to a laboratory at lower level, as we do with ordinary rock specimens. Nor can thin sections in the ordinary sense be ground and examined under the microscope. A refrigerated laboratory in the high alps, at a level corresponding to that of avalanche formation, was essential. Specimens in undisturbed positions had to be collected with the greatest care by means of cylindrical snow sondes (hollow steel-tubes of suitable size), and submitted to immediate examination. The sondes are pushed or driven in vertically or parallel to the surface so as to obtain samples of different layers of snow or of one only. Special techniques for making thin sections, analyzing grain size fractions with sieves and a sedimentation balance, and for determining the weight of unit volume, the penetrability and cohesion had to be worked out. Within a trial field, profiles had to be made constantly and at regular intervals and reduced to time profiles, so that all changes in the snow cover could be studied on the spot. One of the chief results is the following:

During the snowfall itself, but especially after the snow has been deposited, a radical alteration takes place which, in the course of time, changes the character of the snow cover. The sediment undergoes metamorphism and develops a completely new fabric with new mechanical properties. New snow is transformed into settling snow, settled snow and, finally, old snow of variable character and with changing degrees of composition and adhesive resistance of the grains. A stratification of the deposit, depending on the climatic condition during and between the individual falls of snow, is brought about, and in many cases the course of alteration can be predicted well in advance. This is important as the formation and type of avalanches depend on the character of this stratification. The study of the behavior of the alpine winter snow cover thus becomes the most impressive example of the metamorphic processes to which so many rocks owe their appearance. In this case, it has the inestimable advantage of unrolling itself before our own eyes.

The primary cause of snow metamorphism lies in the possibility of settling, in the instability inherent in the complex forms developed by the snow crystals during their all-too-rapid dendritic growth. Once deposited and covered, the crystal of new snow is surrounded by an atmosphere possessing only average saturation with water vapor. The sharp points and strongly curved surfaces do not completely retain the molecules which again go into the vapor space or creep along the surfaces to attain other positions. Thus, the surplus of surface energy which distinguishes skeletal forms from closed polyhedral single crystals manifests itself. A redistribution of matter leading to a reduction of total surface area sets in and can be followed by cinematography. This constitutes the formation of granular snow. It proceeds faster at higher temperatures than at lower ones; hence, only at the latter can powder snow retain its loose felted character for considerable time. Near the freezing point, the effect of surface tension seems to manifest itself up to surface radii of at least 100 to 200 microns. Fallen snow has, therefore, the tendency to settle and recrystallize and to attain an increased cohesion due to the loss of the felted texture. The rate of change in frost is a function of the temperature, which itself varies in the snow cover from point to point and also in time.

Daily observations of standard cross sections in a given snow test field give a detailed insight into the variation in temperature on, within, and below the snow cover. As an average of all daily observations, it can be stated that in the high mountain region the surface of the snow normally shows the lowest temperature (often —10 to —30 degrees C.) at about 8 a.m.; with increasing depth in the snow cover, the temperature of the snow rises rapidly at first and then with increasing slowness. The average temperature of the air measured 10 m. above the snow surface often corresponds to that of the snow at a depth of 10-20 cm. below the surface. The temperature of the boundary layer between the hard pan and the snow cover varies in the studied regions between 0° and —2° C. during the whole winter. The temperature gradient in the snow cover thus depends on the air temperature, the daily variations of this temperature, and the thickness of the snow cover and its exposure. The temperature of a given layer changes its value during the day and night, according to weather conditions and also when a new snowfall takes place which enlarges the total thickness. The mean density of the whole snow cover rises after the first abundant snowfall, but the different layers tend to different densities depending on the snow conditions, the meteorological phenomena, and the different temperatures within the cover. In the Swiss Alps above 2000 m., the total weight of the snow cover reaches a maximum towards the end of the winter, because during mid-winter the melting point is never reached at points situated somewhat below the snow surface. The transformation of new into old snow of generally fine-grained character takes place without the participation of a liquid phase. Only as the result of intense sun heat can snow melt superficially, later to be refrozen into a so-called sun crust resembling firm snow lying on softer snow.

These relatively thin surface layers of a fine weather period without snowfall are generally well consolidated, but nevertheless by no means stable as regards form. This is a consequence of the different orientation existing between the grains rounded by melting and the mass in which they are embedded. Other surface layers are formed by sublimation after nights of low temperature and clear sky, and are called surface hoar. Other films or crusts of hard snow lying upon a softer layer, like spring crust or rain crust, generally occur in the higher mountains only in the spring. Below these small layers, the settling and metamorphism to ordinary granular old snow goes on. If a new snowfall now covers the sun crust or a surface hoar layer, all conditions under the new burden are changed and the metamorphism of the deeper layers makes a new start. Often, by collective crystallization, the unstable small surface layers of the earlier snow layers produce grains of large, fairly idiomorphic, cup-shaped appearance, whose cohesion is practically nil (loosening of the crusts). Such formations are called depth hoar and behave like quick or shifting sand. It is evident that the comparatively high temperature prevalent in layers near the earth's surface will often also cause them to undergo changes of this kind. But generally, during the first days after a heavy fall of new snow, all that takes place is a loosening of the upper part of the old layer and a progressive hardening and consolidation with recrystallization just below these crusts.

In this way, and depending on weather, exposure, temperature, snowfall, and the type and rapidity of the metamorphism, a banded structure or stratification tends to develop during the winter, which is due to alternating layers of snow showing varying compactness, strong and weak cohesion, and different grain size. By taking driving diagrams with a pile driver or rammer, we can easily distinguish between the layers of different cohesiveness, and, by studying one layer during the whole season, we obtain an insight into the course and rate of metamorphism and can compare this with the meteorological conditions.

The different layers, all of the character of crystal aggregates, have in common with all solids a certain tensile pressure and shearing strength as well as plasticity. When, as in the mountains, the snow is deposited on very uneven surfaces, the influence of gravity naturally produces strong strains within the sloping snow cover. In a porous aggregate (even old granular snow has pores amounting to 50 per cent of the volume) filled with a gas phase under hydrostatic pressure, the distribution of these strains will be a peculiar one and will also exercise an important influence on the processes of recrystallization and mechanical deformation. At first, a redistribution of material, tending to ease the strains, will take place. One special manifestation of this tendency is a rise in the vapor pressure near the surface of grains under stress to values above that of normal saturation. The surface of contact between different grains is only a very small part of the total surface. This may lead to crystallization from the supersaturated vapor phase, whereby a redistribution of matter within the snow mass may ensue. The same phenomena are found in stressed silicate rocks under higher temperatures and are one of the reasons for dynamometamorphism. In another special case, crystal plasticity within and between the grains may become operative and result in yielding by translation to the stress. In the first instance, lattice layers of individual crystals are translated along each other. Slipping takes place on the crystallographic plane (1,000) and along certain crystallographic directions. In the second case, crystals, as a whole, glide or rotate over other crystals and the combination of intra- and intercrystalline plasticity is characteristic of a polycrystalline aggregate. The inherent tendency to lattice translations in snow crystals (and hence their crystal plasticity) and also the mobility of the grains are, to a remarkable degree, dependent on temperature. This is proved by the creaking of snow on cold days, due to increased hardness and brittleness. Near the melting point, however, translations and gliding produce a complete rearrangement combined with a regulation of the fabric similar to that observed in the annealing of metals and in crystalline schists.

On the whole, however, the behavior of snow in the neighborhood of and somewhat below 0° C. under static load is in many respects more analogous to that of a viscous liquid, with high internal friction. In connection with sudden dynamical forces, the effects of elasticity are also important. The movements of snow layers on a slope can often be treated like the surface flow of glaciers. The most important difference is the porosity of the snow, the more homogeneous layered structure, and the freedom of the fabric which ensures a greater mobility of the grains themselves.

Of course, the micromechanism of creep and flow or, generally speaking, of plastic deformation in this transparent polycrystalline material, is very complicated and not yet known in detail. But to obtain a general survey, the effects can be summed up in terms of the coefficients of viscosity or toughness indices. It is then seen that the viscosity generally increases from felted snow to fine-grained and coarser-grained snow. The viscosity increases with sinking porosity and, to a large measure, with sinking temperature. It seems that the order of magnitude of the coefficient of viscosity (between 0° and —40° C.) is 10^{10} to 10^{15} for different dry snow layers, corresponding to a time of relaxation of 10^1 to 10^6 seconds.

The modulus of elasticity for granular snow at the temperature of —10° C. is of the order of magnitude of 10^5 kg. per square meter. For compact ice, the value of 10^8 kg. per m.^{2} has been determined.

In order to obtain information about all the mechanical properties of the different layers of a snow cover, methods and terms similar to those used in metallurgical engineering and technical petrography must be employed. Undisturbed test specimens of snow in cylindrical form are subjected to the action of progressively increasing load or tensile pull until they break. In a rotational apparatus, values of the "strength" are obtained. The deformation of the polycrystalline aggregate and the response of a snow layer to applied forces must be studied. With creep test methods, the effects of temperature, porosity, and microstructure, of crystal size on deformation and on the resistance to deformation can be studied. On slopes with different angles of inclination, the creeping-pressure is measured, which, of course, is necessary for the construction of the different types of counter-avalanche defenses (rows of retaining-walls or isolated "resistance elements"). It is found that the strength against tensile stress is greater if porosity and temperature are low. Maximum values are often found in fine-grained snow and lower ones in felted, newly fallen snow or coarse-grained old snow.

Under natural snow conditions, the various effects will be superimposed on each other and contribute to the general and differential plasticity and strength of the layers of the snow cover. The varying properties and anchorage of the different layers in respect to the lower ones tend to disturb the uniformity of the phenomena of creeping which can be observed on slopes, and sliding friction between the various metamorphosed layers assumes different values. Naturally, various rates of creeping set in and cause longitudinal strains, generally also a zone of tension in the upper parts and one of compression in the lower parts of an inclined snow field, while along firmly anchored lines zones of shearing occur. Ultimately, when the strains in a particular layer have risen to values near the yielding point, an extremely labile equilibrium remains. Very insignificant factors such as a small breakaway of snow or a new snowfall with its pressure excess, now suffice to produce sudden enormous increases in the strains and lead to the formation of avalanches. In the case of the particularly dangerous snow slab avalanches, consolidated upper layers glide over others which, loosened by metamorphism, act as a lubricant over a glide plane. By taking, every day, sonding tests with the rammer and taking the weather conditions into account, an estimate can be made of the likelihood or otherwise of the occurrence of avalanches which are dangerous chiefly on account of the mass of material set in motion. In the case of a dangerous situation, not only can warning be given, but the possibility exists of producing avalanches artificially with explosives or by shooting down the snow in small portions before a great accumulation has taken place. In the last war, an avalanche service was introduced in the Swiss army with the effect that the concentration of troops in the Alps for defense work was carried out with practically no accidents.

Many important details of the development of slip surfaces can already be sharply formulated, but the indications made will suffice to show that a study of the conditions for the metamorphism of the snow cover, and the course taken by it, together with a thorough knowledge of the mechanical properties of the structurally different sorts of snow, is an essential prerequisite for a scientific treatise on avalanches. Laboratory work entailing an exact characterization of every kind of snow, and determinations of the gradation of grain size (elutriation and sedimentation balance), packing, and pore space in relation to the mechanical properties are necessary.

Another very interesting part of the researches relates to the thermal economy or heat balance of the snow cover. It comprises the sum of gains and losses of all heat currents which pass the boundaries of the layers (heat current through the base of the snow, conduction and radiation within the snow cover, radiation balance on the surface, exchange of heat with the surrounding air by conduction and convection, exchange of heat caused by condensation and evaporation processes).

Especially, the radiation balance on the surface is dependent on the vertical component of the solar radiation, the long-way heat radiation coming in from the atmosphere or the clouds, and the outgoing radiation from the snow cover.

The radiation from sun and sky which reaches the snow cover is partly reflected (often more than 50 per cent) and partly transmitted by absorption. The absorption coefficient rises as the settling and compactness of snow increases. All bodies become athermanous to radiation at a certain thickness; for snow, a thickness of about 50 cm. suffices. The reflecting power was measured by means of a suitably mounted thermopile and a photoelement. Knowledge of the reflecting power permits the calculation of the amount of radiation absorbed per square centimeter of the snow cover, when the values of the sun and sky radiation incident on the horizontal surface are given.

In spring, the intensity of the radiation, the length of the days, and the height of the sun and the absorbing power in the upper part of the cover greatly increase, while the reflecting power of the snow simultaneously decreases. From this, it is clear that the amount of radiation energy absorbed daily by the snow cover must strongly increase from mid-winter to spring. In winter itself, the snow cover is hardly affected by radiation during fine weather, while toward spring it is melted, the process of melting chiefly taking place on the surface of the snow.

As a rule, the entering radiation predominates during daylight and the radiation balance is negative at night. The total exchange is, of course, greater under a clear sky than under cloudy conditions. At night, the temperature of the radiating body falls and the amount of the negative radiation balance sinks from evening towards morning. Snow possesses low absorbing and high reflecting power for short wavelengths. On the other hand, it absorbs long-wave radiation almost like a black body. Its thermal conductivity is low. Snow thus belongs to the bodies which at all times show a low radiation exchange. Ice lies between water and snow in respect to the radiation balance.

The amount of heat, in calories, necessary to raise the temperature of a snow column of 1 cubic centimeter section to 0° C. without melting the snow is called the *cold content*. This amount depends on the thickness, the fabric and porosity of the snow column, and the temperatures within the column. Generally, the heat of fusion is 20 to 50 times greater than the cold content in a snow cover at 1500 m. above sea level in winter time.

From the thermal economy it follows that the mainly dry metamorphism of the alpine winter snow cover is complicated towards spring by melting and thawing processes. Water soaks the snow. This can be the cause of wet snow avalanches and ultimately leads to the complete disappearance of the seasonal rock. In the region of so-called perpetual snow, similar processes (in conjunction with the temporary presence of the liquid phase) lead to the formation of firn and, finally, to the practically compact glacier ice. The individual grains first melt at the corners and edges so that further settling and closer packing result. Old firn-ice has an absolute porosity of only 50-33 per cent, while glacier ice is often altogether lacking in communicating pores. The horizons of dirt appearing in firn ice are produced by rock fragments transported by the wind and deposited on the snow cover. These fragments may, during melting, sink in to a depth of several centimeters. Bands of a more compact nature appear as so called "blue bands." They are, in a sense, cementation zones deriving from melt water percolating downwards and often possess a regulated fabric, the chief axis of the ice crystals being generally perpendicular to the banding. It is interesting that the apparent viscosity is many times greater in frozen snow, possessing no preferred crystal orientation, than in a polycrystalline block, where all the crystal glide planes are more or less parallel to the direction of shear.

Many details of what takes place still have to be worked out, and even our conceptions on the movement of glaciers must be revised from the standpoint of crystallography and the modern science of plastic flow in polycrystalline aggregates. To this end, not only the snow and avalanche station on Weissfluhjoch, but also the research institute on Jungfraujoch are engaged in studying this much more drastic type of metamorphism by all the methods of modern science. Swiss glaciologists are working together with the Cambridge glacier physics committee. It can confidently be expected that the snow and ice rocks will, in spite of their simple chemical composition, also furnish a valuable contribution to our knowledge of the process called ultrametamorphism, to which, at present, petrographers are devoting much attention. Rocks originally formed by the consolidation of magmas or by sedimentation are, during the course of major tectonic disturbances, subjected to totally new conditions whereby they also undergo changes. Under the influence of rising temperatures they may, in full analogy to snow, wholly or partially revert to the molten state. Further changes in the outer factors may then lead to a renewed formation of solid crystal aggregates. The mutual relationships between crystal and melt can, in part anyway, be elucidated by the study of wet snow, firnification, glacier formation, and glacier movement. They provide a check on and a correction of the theories and conceptions advanced by the students of magmatite formation.

It may, thus, truly be said that the science of snow, while using the methods of crystallography and mineralogy and physics of the solid state, provides many a valuable insight into the problems of these very sciences. For countries in which villages and towns have arisen in the high mountains, in which roads and power stations are being built, in which winter sports flourish and the army has to defend mountain passes—for these countries, efforts to avoid the dangers of avalanches are a vital necessity. With this end in mind, the research station on Weissfluhjoch near Davos in Switzerland was founded. Some aspects of the work done by our collaborators, especially by Professor Bader, Drs. Morikofer, Winterhalter, Haefeli, and de Quervain, Engineer Bucher and the assistants Thams, Eckel and Roch have been recorded in lantern slides and a short film for demonstration purposes.

SECTION OF BIOLOGY

ESSENTIALITY OF CONSTITUENTS OF SEA WATER FOR GROWTH OF A MARINE DIATOM

By S. H. HUTNER [*]

The growth requirements of marine organisms higher than bacteria are poorly understood. Knowledge of the ionic relations of marine forms may assist in testing the theory that the electrolyte balance necessary for the functioning of mammalian tissue is a reflection of the pattern imposed on protoplasm by its origin in the ancient less saline sea—a hypothesis fascinatingly sketched by Baldwin.[1] The inshore diatom *Nitzschia closterium,* isolated in England nearly forty years ago, has been studied intensively, and was therefore chosen as the starting point of this investigation. The relation of this older work to general oceanographic problems has been summarized by Harvey.[2]

Only guide-post data are presented here, but as the *methods* being developed to evaluate the role of each constituent of sea water appear widely applicable, emphasis will be placed on the theoretical foundations of the procedures, and experimental results are cited mainly to illustrate these methods. The conclusions drawn from these results are highly tentative. Much information has accumulated, but more quantitative data are needed before clear-cut principles can be formulated.

Methods. A bacteria-free subculture of the classical strain was obtained from Professor E. G. Pringsheim. Stock cultures were maintained on 1 to 2 per cent sea salt (a convenient substitute for diluted sea water)—peptone agar slants, pH 7.1 to 7.7, in screw-capped tubes. Peptones of the tryptic digest of casein type (trypticase or tryptone) allowed heavy growth at the 0.1 to 0.5 per cent levels. Cultures remained viable for at least a month when stored in darkness at 6° C. Experimental cultures were grown in 25-ml. Pyrex Erlenmeyer flasks, plugged with Pyrex glass wool. Illumination and the proper temperature (18° to 20° C.—this is close to the upper limit) were secured by placing the flasks on glass trays in a refrigerating incubator (Central Scientific Company) equipped with two 20-watt "soft white" fluorescent lamps. Growth was rapid under these conditions, and was measured after 25 to 35 days. Otherwise the procedures were similar to those previously described.[3]

Results with Citrate. Work with enriched synthetic sea water media is hindered by the formation of precipitates. These, by absorption, remove essential elements, as indicated by the repeated observations of other workers that filtered solutions do not support growth. In devising substitutes for sea water, it early became apparent that calcium, magnesium, and iron were required in rather high concentration, each inducing precipitates at pH 7.0 and higher. The use of citrate overcame this difficulty. Citrate forms soluble chelate complexes with many elements, but in so doing removes them from the field of action. The formation of the calcium citrate complex is illustrated in FIGURE 1. As the citrate concentration is increased, greater proportions of certain essential elements are bound, and it becomes necessary to adjust upwards the concentrations of these elements. By supplying an excess of all essential elements save one, one may then plot the increment of this element required to compensate for each increment of citrate. Then, by extrapolation to zero citrate, it is possible to arrive at a value closer to the *absolute* requirement for a particular element. This procedure applied to calcium and magnesium is illustrated in FIGURE 2. It is impractical to set up experiments with no citrate because of the formation of precipitates already

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[*] Haskins Laboratories, New York, N. Y. This paper, illustrated by lantern slides, was presented at the meeting of the Section on January 12, 1948.

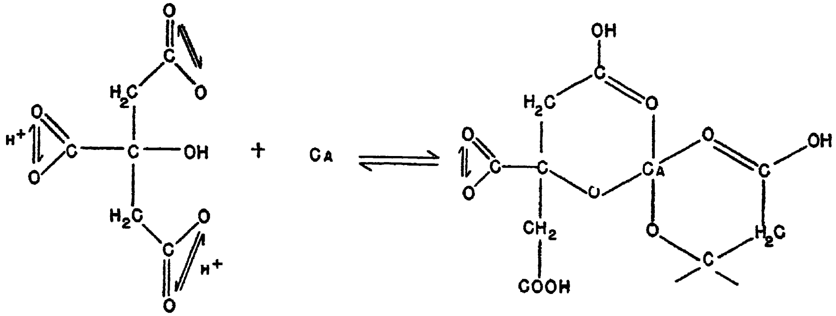

FIGURE 1 Possible manner of formation of chelate rings in the calcium citrate complex One calcium ion may coordinate with more than one molecule of citrate In this and the subsequent figures, only a limited effort is made to depict interatomic bond distances accurately

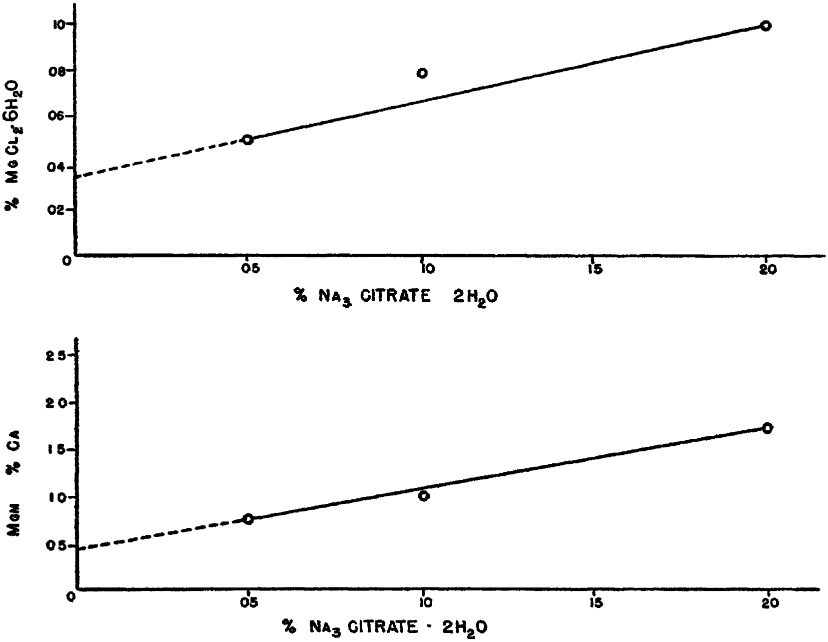

FIGURE 2. Calcium and magnesium requirements as a function of citrate concentration.

mentioned. The points plotted represent the amount of metal (or salt) that must be added to restore half maximal growth.

Before accepting these "absolute" values, it is necessary to define the experimental conditions rigorously. One way to test the accuracy of these values, obtained by what may be termed the citrate extinction technique, is to determine whether other complex-forming reagents, yielding curves different in slope, yield the same intercept as the citrate curve when extrapolated to zero reagent. For this purpose, the tartrate, oxalate, and other complexes are under investigation. Additional requirements for rigorous accuracy can be posed:

different non-toxic salts of the element in question must furnish the same intercept; furthermore, when a salt of the element is purified by at least two independent methods, both must furnish the same intercept. Meeting these conditions entails much tedious work but appears to be necessary to keep impurities in commercial "chemically pure" substances from seriously influencing the results. The satisfaction of these requirements for accuracy appears well on the way to fulfillment for calcium and magnesium, but is more remote for iron and manganese. Here, the situation is complicated by simultaneously induced deficiencies in molybdenum and other trace elements. Methods are being devised for each of the constituents of the culture medium, and it is likely that these purified materials will lead to reproducible results. The citrate extinction technique is an extension of the procedure employed by Hopkins and Wann[4] for determining the iron requirement of the green alga *Chlorella*. TABLE 1 shows the type of medium used for obtaining "extinction" data. It allows very heavy growth. The concentrations of nutrients are adjusted to the level of citrate shown in the table.

TABLE 1

SYNTHETIC MEDIUM FOR *Nitzschia closterium* *

K_2HPO_4	0.04%
NH_4NO_3	0.05%
Na_3 citrate.$2H_2O$	0.1%
Na metasilicate.$9H_2O$	0.005%
NaCl	0.2%
$MgSO_4.7H_2O$	0.25%
Ca (as $CaCO_3 + HCl$)	3.5 mg.%
Fe (as $Fe + HCl + HNO_3$)	0.5 mg.%
Boron (as H_3BO_3)	0.05 mg.%
Zn	0.005 mg.%
Mo	0.005 mg.%
Cu	0.005 mg.%
Mn	0.00005 mg.%

* pH 7.2-7.5. Because of toxicity, Mn, while essential, must be kept low.

On consideration, the citrate extinction technique is recognized as a special case of the method of competitive inhibition now widely exploited as a tool for finding new antimicrobial agents. But here the interference is with essential inorganic metabolites rather than with organic metabolites, and the mechanism is one of interception of the essential element rather than "jamming the lock" based on structural homology. This method provides a means of minimizing the importance of chemical contamination of the environment, by exaggeration of the requirement for a specific element or group of elements. Analogously, the discovery of the ubiquitous and highly potent growth factor, *p*-aminobenzoic acid, would have been much delayed if it had not been brought into the open as the most powerful antagonist of the sulfonamides. If there are essential trace elements yet to be identified, this method might facilitate their discovery.

The Sodium Chloride Requirement. Like most marine microorganisms, *N. closterium* poorly tolerates solutions more hypertonic than sea water. As the citrate concentration was increased, it was possible (and necessary) to decrease the NaCl to low levels, and even virtually eliminate chloride. It was not possible to obtain growth with concentrations of sodium below 0.02 per cent or so, a finding in harmony with some older conclusions.[5] A medium developed for this series of experiments is shown in TABLE 2. Comparison of it with the medium of TABLE 1 indicates the liberties permissible in abandoning sea water media.

TABLE 2

SYNTHETIC MEDIUM FOR DETERMINATION OF SODIUM CHLORIDE REQUIREMENTS *

K_3 citrate.H_2O	0.15%
NH_4NO_3	0.04%
$(NH_4)_2HPO_4$	0.03%
$MgSO_4.7H_2O$	0.3%
ethyl silicate $[(C_2H_5O)_4Si]$	0.004%
Fe (as $FeSO_4.7H_2O$)	0.5 mg.%
B (as H_3BO_3)	0.1 mg.%
Mo [as $(NH_4)_6Mo_7O_{24}.4H_2O$]	0.001 mg.%
Zn (as $ZnSO_4.7H_2O$)	0.001 mg.%
Mn (as $MnSO_4.H_2O$)	0.001 mg.%
Co (as $CoSO_4.7H_2O$)	0.01 mg.%
Ca (as gluconate)	3.0 mg.%

* pH 7.4. The addition of Na_2SO_4, 0.06%, allows heavy growth.

Preliminary Conclusions as to Ionic Balances. The values in FIGURE 1 for the calcium and magnesium requirements are much higher than those usually found for non-marine organisms. The calcium requirement is of the order of that of the land plants, and may prove deeply significant in view of the importance assigned to calcium in controlling the permeability of all cell membranes.

Differential Permeability and Ion Accumulation. Many organic compounds bind metals tightly by formation of chelate rings. The metal is held in a ring by forces similar to those stabilizing aromatic rings.[6] Citrate is comparatively weak in this respect; greater avidity and specificity in binding elements, particularly those of the transitional series, is possessed by compounds imparting a high degree of resonance to the metal-containing ring. Truly extraordinary biological compounds of this type are the porphyrins, where iron is bound in heme compounds and magnesium is bound in chlorophyll. It is increasingly recognized that the maintenance of differential permeability and the accumulation of ions against a concentration gradient is closely linked to respiratory activity. The suggestion has been made that this respiratory activity is required for the formation of ion-binders of the chelate type.[7] Following this line of thought, the surface of the cell may be regarded as studded with receptor patches, constantly regenerated by energy derived from respiration. The *number* of these receptor sites would help determine the concentration of a particular ion that must be present in the medium to keep the cell alive and growing; and the *avidity* or *"stickiness"* of these sites would determine the ability of the cell to compete with various types of metal-binding compounds for the available supply of essential ions. These receptor sites must be present to abstract such elements as iron and molybdenum from sea water where they occur

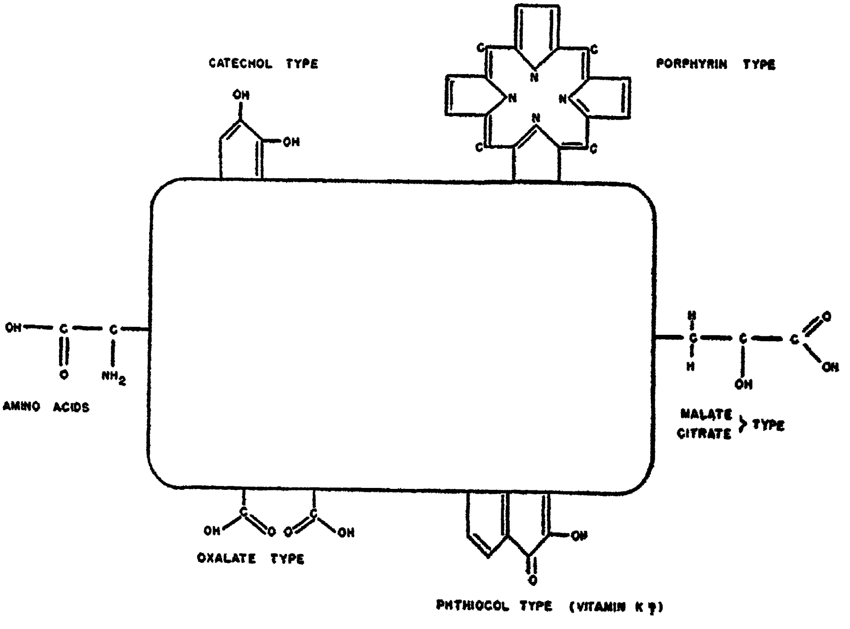

FIGURE 8. Cell with hypothetical metal receptorson the surface.

in almost immeasurably low concentration. The remarkable sensitivity and specificity which make certain chelating reagents so useful in the analytical chemistry of trace amounts of many metals, hint at the employment by the cell of this type of compound for similar purposes. Many components of the cell other than porphyrins and citrate are more or less effective metal binders: *e g.,* α-amino and α-hydroxy carboxylic acids, glycerol, and other compounds with *ortho* configurations of potentially chelating substituents. This concept of the nature of the receptor sites for metals on the cell surface is diagrammed in FIGURE 3. Immunologists may recognize in these ideas an invocation of the shade of Paul Ehrlich.

The competitive action of the bulky hydrophilic citrate radical likely is exercised primarily at the cell surface. Other competitive reagents may be employed for a systematic probing of these hypothesized receptors. Each such reagent may be viewed as competing with its structural counterpart (or, perhaps, more accurately, its electronic counterparts) embedded in the cell surface. Thus *o*-phenanthroline (FIGURE 4) binds iron and the adjoining transitional element of the periodic table in virtually the same kind of linkage characteristic of porphyrins. The question sometimes arises as to whether this competitive action, as distinguished from irreversible poisoning, takes place entirely at the cell surface, or whether there is also an appreciable penetration of the cell, with consequent interference with metal-catalyzed reactions within the cell. The answer may be sought in a comparison of the activities of pairs of chelators, alike in metal-binding capacity, but widely different in ability to penetrate the cell. Such a pair is 8-hydroxyquinoline and 8-hydroxyquinoline-5-sulfonic acid (FIGURE 5).

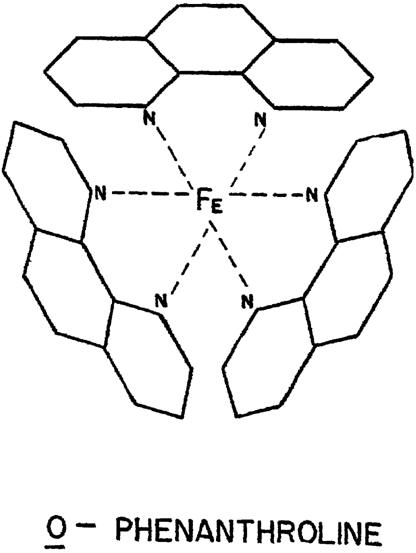

FIGURE 4. The *o*-phenanthroline-ferrous complex. Double bonds in the *o*-phenanthroline are not shown.

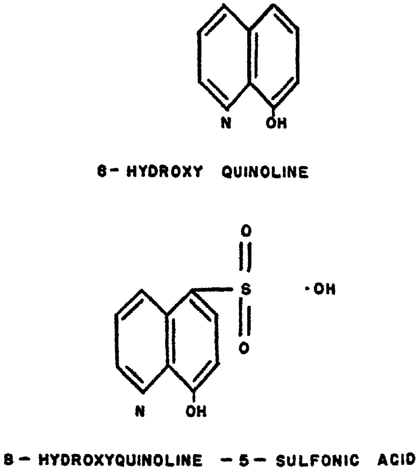

FIGURE 5.

The metal-binding portions of both molecules are essentially identical, but the large hydrophilic sulfonic acid side chain on the one renders it practically unable to penetrate the cell. Respirometric data obtained in conjunction with growth data should be decisive in determining the extent of cellular penetration.

Little can be said at this time about the data obtained by these methods. Exploratory experiments with faster growing organisms such as *Chlorella* and photosynthetic and non-photosynthetic bacteria, indicate that these methods will eventually lead to a quantitative assessment of the requirements of *N. closterium* for iron, manganese, molybdenum, and, very likely, cobalt, and will even allow some well-informed guesswork as to the avidity of its receptors for various elements as compared with other microorganisms. The power and delicacy of this method is borne out by the provocative findings of Albert *et al.*,[8] who noted that cobalt overcame the inhibitory action of 8-hydroxyquinoline on gram-positive bacteria, but that iron plus zinc was required instead to antagonize the action of 8-hydroxyquinoline on gram-negative bacteria.

Diatoms and Other Biological Problems. There are additional objectives in work on *N. closterium* that can be touched on only briefly. Pelagic diatoms require for growth a factor in natural sea water that has, as yet, defied analysis. It is reasonable to suppose that information on *N. closterium,* which does not require this factor,[9] will facilitate investigations of the requirements of these more exacting diatoms. The diatoms belong to a great array of brown-pigmented algae whose energy metabolism has scarcely begun to be studied. Valuable sidelights on such problems as the nature of the energy stockpile built up by photosynthesis might be gained by a thorough-going search for substances permitting growth of this form in darkness. That this substitute for photosynthesis has to be unusually efficient is suggested by the finding that the mere maintenance of turgor in a marine diatom entailed the expenditure of considerable photosynthesis-gained energy.[10]

Viewed broadly, the object of this work is to contribute to the long series of experiments in other laboratories fashioning *Nitzschia closterium* into an effective tool for a wide range of investigations into cell physiology and comparative biochemistry. Because of the ever-present temptation felt by physiologists to confine their experiments to a few well-established laboratory organisms, there is always the danger that far-reaching conclusions may be based on too narrow a choice of living material. The manner in which a fundamental broadening of the theories of photosynthesis and hydrogen transfer waited upon studies of photosynthetic bacteria, complementing the time-honored green forms, is a striking example of the use of new organisms to clarify old problems. It is reasonable to suppose that the diatoms will be an increasingly valuable addition to the armamentarium of the cell physiologist, and thereby enhance the intrinsic importance they already possess as the equivalent of grass in the economy of the oceans.

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SECTION OF PSYCHOLOGY

THE NATURE OF SOCIAL PERCEPTION

By HADLEY CANTRIL[*]

When the term, social perception, was introduced into psychology a few years ago, I, for one, eagerly awaited a clarification of its meaning and anticipated that those using the term had some formulations that might help the social psychologist understand a few of the many mysteries he faces. I assumed that we would be told in just what way social perception was to be distinguished from our up-to-the-now understanding of the nature of perception itself.

So far, however, I know of no satisfactory attempt to clarify the precise meaning of the term that was coined, or to demonstrate its *special* functional significance in concrete situations. The phrase has kept pestering me so much that I concluded my subconscious must be groping for something important in its implications. At the same time, however, I felt certain that, if the meaning of "social perception" were clarified, we would find that it was basically the same as any other variety of perception and would follow the same underlying psychological principles even though there might well be some special characteristic involved that could give us useful insights.

It has become increasingly obvious, during recent years, that the psychology of perception involves a great deal more than analysis of laboratory experiments, no matter how important these experiments themselves may prove to be. The psychology of perception involves the whole field of attitudes and opinions, of judgments and belief. In our *Psychology of Ego-Involvements*, Sherif and I, for example, tried to show how the psychology of attitudes was rooted in the psychology of perception itself and to point out how much the social psychologist concerned with the nature and function of attitudes could learn from the experimentalist's rigidly controlled studies of perception and judgment. Since an understanding of human experience itself can be largely translated by the psychologist as an understanding of perception, its nature and determinants, it is encouraging for any psychologist who takes it upon himself to explain the behavior of men in concrete social situations to see that research into the nature of perception is currently advancing on a number of different fronts.

It is by no means original to point out that perhaps the most basic and crucial problem facing the psychologist is that of the precise causal relationship between perception and man's purposive behavior. In one way or another, it is this central problem that has engaged the attention of nearly every investigator whom most of us would agree to regard as outstanding in the short history of psychology. In spite of this, I am sure we also feel that only a beginning has been made. My discussion of the nature of social perception is offered here as a reflection of my own current groping for a more adequate and a more valid set of higher-order abstractions which may, eventually, give us a better understanding of the important causal relationship between what is traditionally referred to in psychology as "motivation" and "perception."

In the abovementioned volume, Sherif and I quite consciously left this problem hanging somewhat in mid-air. And for the past eighteen months, with what little spare time a teacher is able to find these days, I have been trying to answer in my own mind some of the questions that arise when one starts thinking about motivation and perception. I discovered that the hardest job, and the job that has to be done before one can even begin, is to ask oneself the proper questions.

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[*] Princeton University, Princeton, N. J. This paper was presented at the meeting of the Section on January 19, 1948.

As I began my investigation, I happened to learn almost by accident of the research on this very problem being done by Adelbert Ames, Jr., of the Hanover Institute. A brief discussion with Ames and a few hours with him to introduce me to the demonstrations he has devised during the last few years quickly convinced me that a more complete understanding of the implications of his demonstrations could be of more immediate help to me in straightening out my thinking than anything else I might do. I was, and remain, convinced that Ames is several jumps ahead of the field in his formulations; so much so, in fact, that, as so often happens in the history of science, either the real significance of his work is not seen or, when it is seen, it is regarded as something we knew all along—which I, for one, don't believe we did.

Through the cooperation of the Hanover Institute, a complete set of these demonstrations is now part of the Office of Public Opinion Research in the Department of Psychology at Princeton and a few of the demonstrations, together with a laboratory manual, are being currently used with great success in the laboratory periods which are part of our introductory psychology course.

Some experimentalists who have gone through the demonstrations with me at Princeton have wondered why a social psychologist and someone who is occasionally labeled as a mass movement or public opinion specialist, should be interested in demonstrations which to them seem chiefly concerned with an explanation of how and why we get our experience of depth perception. The answer is, of course, that a clear understanding of perception itself (no matter what the specific perceptual problem may be) has just as much relevance for someone concerned with the nature and significance of attitudes and opinions as it does for someone whose chief interest may be the time error or size constancy. For principles of psychology, once we discover them, must—if they are principles—be applicable to all of man's experience whether this takes place in the laboratory, in the clinic, or in everyday social life.

I have gone out of my way somewhat to pay tribute to the work of Mr. Ames because my own discussions with him and the various research programs now under way at Princeton involving his demonstrations are inevitably reflected in the remarks to follow. I find it difficult, almost impossible, to know where to begin a discussion on the nature of social perception or, for that matter, any other topic in psychology. I seem to start my course in social psychology with a different topic every year, hoping that some day, through experience, I shall find out what really should come first for purposes of communication and understanding. So many variables are interrelated. I shall, then, have to touch briefly here on a number of variables involved if I am to communicate to you what seems to me to be the proper understanding of the term, social perception.[*]

The Nature of Perception. Since "social perception" can be understood only in so far as we understand the nature of perception, it will be necessary first of all to indicate what formulation of perception seems to make the best sense. If this formulation appears to be somewhat radical, it may be because the approach is 100 per cent functional, not limited to perception as a study of a series of quantitative relationships. The demonstrations, together with other evidence from experimental and social psychology, seem to point to some such interpretation as the only one that will eventually give us some clarification of the role of perception in purposive behavior. Here I can only sketch in barest outline some of the characteristics of perception that must be taken into account in any valid and adequate interpretation.

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[*] A more detailed elaboration and a more general formulation of the present discussion will be found in: Cantril, H. Understanding Man's Social Behavior: Preliminary Notes. Office of Public Opinion Research, Princeton, N. J. 1947. Some of the present discussion concerning the nature of perception and social perception is taken from that little book.

First of all, it seems clear that any perception is an awareness that emerges as a result of a most complicated weighing process an individual goes through as his mind takes into account a whole host of factors or cues. It must be emphasized at the very outset how tremendously complex even the simplest perception is—for example, the perception of a star point. For it can be demonstrated that, in perceiving a star point as such, a whole host of indications are weighed and integrated to give us our final experience. Among these indications are those related to the environmental conditions just prior to and during the visual experience of the point, together with all the past experiences we have had with similar stimuli which have built up in us a sort of statistical average used as a frame of reference for our interpretation of the concrete stimulus of the moment.

We must also remember that the integration of all these factors is accomplished in a fraction of a second and is, more frequently than not, entirely unconscious. As the stimuli from outside become more complicated, running, for example, from a star point to lines, to three-dimensional objects, to objects in motion, to persons, or to a newspaper headline describing an international situation, the number of factors which must be integrated runs into many thousands.

At the turn of the century, Mayer and Orth apparently surprised many of their colleagues when they pointed out that a judgment results from mental processes that are *unanschaulich*. A few years later, in 1903, Orth used the term *Bewusstseinslage* to help describe the condition or the set the mind assumes in coming out with a correct intuitive judgment, the actual details of which elude introspection. Ach's term *Bewusstheit* described essentially the same "impalpable" aspects of consciousness. These were bold and significant statements. But while the Würzburger professors did, in a sense, show us what perception was not, their approach seemed to end in a blind alley, for no new positive understandings emerged as to what perception actually was.

From our present vantage point, the observations of Helmholtz concerning the nature of perception take on special significance. In the third volume of his *Physiological Optics*[3] with its chapter 26 entitled "Concerning the Perceptions in General," Helmholtz makes it quite clear that our perceptions are based not only on past experience but are discriminative and accurate largely in so far as "they are useful in enabling us to recognize external objects" (p. 6). Throughout this chapter, Helmholtz emphasizes what he regarded as a fact: that perceptions function only in so far as they serve the organism in the process of adjustment.

It appears that the significance of Helmholtz's contribution in this field was largely lost sight of in the great scientific arguments which immediately followed his death and continued for years on the relative merits of his empirical theory and Hering's nativistic theory. The force of Helmholtz's arguments can now be concretely experienced in the Ames demonstrations.

In addition to the fact that perception seems clearly to result from the weighing and integrating of a whole host of factors and that the factors introduced into this weighing process are those which have been learned largely unconsciously from past experiences, it appears that a good deal more emphasis must be given now to other characteristics of perception implicitly recognized by Helmholtz but not explicitly elaborated by him. The first of these factors is what we may call "purpose," a word about which I shall say a bit more later on. For it seems obvious that the process of reaching a value-judgment, the unconscious weighing that man's brain is able to make of numerous cues during a fraction of a second, is by no means a random and chaotic procedure.

The weighing process, resulting in a perception, goes on for a purpose, whether that purpose is seeking food, adjusting one's footsteps to a curbing, picking up a book, reading or underlining certain passages in a book, joining some gang or group, or accepting or rejecting some political ideology.

In addition to the variable of purpose, it seems clear that this unconscious weighing of cues built up or learned from past experience is brought into operation when some stimulus is related by the organism to some purpose. It is also obvious that purpose can only be achieved through action.

In brief, then, perception itself is part and parcel of purposive activity. It is an emergent co-product of purposive activity and can not really be understood except in that context. We may define a perception, then, as an implicit awareness of the probable consequences an action might have for us with respect to carrying out some purpose that might have value for us.

From this definition of perception, it follows that what we have in mind or what we perceive "now" is almost invariably related to our expectancies for the future, whether this future is the probable consequences of our actions within the next fraction of a second or the probable consequences our actions of the "now" may have for us twenty years hence. Expectancy is our present reaction to the future in terms of what will happen to us if we do (or don't do) certain things now. Stated more precisely, expectancy is the psychological co-product of emerging purpose when related to potential action in carrying out that purpose in concreteness. It is within a context of expectancies that we perceive, judge, feel, act, and have our being. Your own introspection will show you that nearly all positive values of life are related to your expectancies. Without expectancies, you cease to function as a human in being, become static, and are no longer plugged in with the changing, emerging concreteness around you.

It should, perhaps, be pointed out that when the role of expectancies is added to the role of past experiences in determining perception, the "immediate present" becomes much less significant than it is considered to be by gestalt psychologists. Without ignoring the role of so-called "autochthonous" factors in some perceptual groupings, the point of view outlined here requires the introduction of other factors in accounting for the *why* of what gestalt psychologists term "relational determination" and in explaining *why* various cues such as brightness, size or overlay function as they do to help give us our sense of distance.

Perception, as such, simply cannot be understood unless perception in any concrete situation is regarded as a process that occurs in a time continuum that connects the past with the future. Unless the dimension of time, including the experience of the past and the expectancies of the future, are included as basic factors in determining the emergent perceptions we experience in everyday life, gestalt psychology seems to be put in the strange position of denying one of its own fundamental premises, *viz.*, that the whole is greater than the sum of its parts.

Action, purpose, and expectancy are, then, all intimately tied to each other as factors involved in any perception. When they are tied together, it becomes a relatively easy matter to account for the feeling of surety or lack of surety that accompanies perception. We experience a sense of surety in our perceptions if they have in the past proved to be reliable guides to purposive actions. Actions that persist, such as instinctive behavior, reflexes, habits, or conscious and deliberate efforts, do so because they have proved their adaptive value. On the other hand, we experience a feeling of insecurity, an overtone of anxiety when we are able to make only a low prognosis of the probable consequences of our future actions with respect to carrying out a purpose. Furthermore, we experience a lack of surety when conflicting cues are introduced in the weighing process. This can be very dramatically shown in the Ames demonstrations and is experienced in everyday life when we encounter new environments in which we have not yet learned, through concrete action on our part, the significance of various cues. Thus, the city dweller will experience lack of surety when he takes his first hunting trip in the big woods, the landlubber will find it difficult to estimate the size or distance of ships that pass his own in mid-ocean, a Southern gentleman may not know exactly how he should behave in the presence of a deeply pigmented man who wears a high turban and speaks a strange language. Surety of perception is also obviously affected by surety of purpose, by the relative significance to the individual of alternative goals.

Surety of perception, for whatever reasons it is established, is, of course, reflected in the speed and consistency of judgment and action. The effect of the surety of judgment on reaction time was demonstrated, for example, in an experimental study made by the author[2] in 1931 which showed "the positive correlation between an individual's degree of acceptance of a particular evaluative attitude and his speed of association time to words which have reference to that attitude" (p. 92). The effect of the surety of judgments on the consistency of behavior was shown, for example, in Jack's study of preschool children where he found that systematic training in the use of play materials and the psychological co-product of greater self-confidence would produce consistently ascendant responses in children formerly non-ascendant in the same situations.[5]

It is a well-known fact, established in numerous studies in a variety of fields, that the characteristic selectivity and discrimination involved in any perception is dependent on the referential framework built up from concrete past experiences. But the functional role of these perceptual frameworks as guides to action in carrying out the organism's purposes has by no means been sufficiently clarified.

It is, of course, in this connection that the Rorschach tests, the experiments of Murray and his co-workers on apperception, the studies of Murphy and others on autism, and the more recent studies of Bruner and his associates are significant. When we say that all of these studies unequivocally demonstrate how our experiential world, our perceptions, are affected in a major way by our purposes, what do we mean? We can only mean that, if we have learned from our past experience—which includes our own action in checking the reliability of our perceptions—that stimuli have the possibility of various kinds of significance (and there are apparently precious few, if any, stimuli which do not have such a possibility), then we will perceive the particular significance in the stimulus which has been consistently related to the achievement or the denial of our purposes. The perception of this particular significance, if not checked or counteracted by other cues, will lead to the familiar "distortions" of the "objective" external stimulus.

This phenomenon is found in a wide variety of everyday life situations as well as in laboratory experiments or in the psychiatric clinic. Our understanding of this and all other perceptual phenomena will obviously be precise only in so far as we can clarify what we mean by "purposes." It is quite impossible to go into any detailed discussion here of what I mean by the term. However, I must at least point out that I refer not only to man's instinctual needs or physiological tension systems or system, but also to specific and learned status or ego-strivings. The specific directions an individual's ego-strivings take as he tries to satisfy instinctual needs or physiological tension systems, can be analyzed in terms of their functional significance to the individual through a knowledge of his developmental history. Here, the ethnologists, the sociologists, with their excellent case studies, and the child psychologists immeasurably contribute to our understanding. Laboratory experiments, such as those of Sherif which demonstrated how the significance attached to a stimulus could be affected by experimentally induced group norms, substantiate the interpretations we can arrive at from more everyday life analysis.[6]

Yet, without in the least minimizing the importance of finding out all we can about man's instinctual needs, his tension system and the way in which men in different societies or sub-groups acquire specific ego-strivings and specific directions for their behavior, I feel certain that these tasks are *relatively* easy and that, even when accomplished more satisfactorily than they are now, the psychologist will still be left without an adequate motivational scheme. The *why* of much of man's most characteristic behavior, including his status and ego-strivings, will still be unexplained. I am convinced, therefore, that psychologists concerned with an understanding of what purpose is must seek out all the information they can from the most competent biologists and geneticists concerning man's unique pattern of growth with the emergent characterisics that have appeared in the course of evolution. I feel confident that, once man's pattern of growth is understood in the distant future, it will be found to be composed of a great deal more than instinctual needs or physiological tension systems. In addition to these, and in addition to man's obviously superior capacity for rational and abstract thinking, for concept formation, for the construction and use of artifacts, I have a feeling that *the* characteristic of man will be revealed as an urge for improvement, a desire to develop directionally. Only when this emergent characteristic is taken as part and parcel of man's distinctive pattern of growth can we satisfactorily understand man's purposive strivings. Our explanation must, of course, be entirely naturalistic.

In this connection, it gives a psychologist some comfort to read a statement by the eminent biologist, C. Judson Herrick, that "the thing that is most distinctive about man is the *pattern* of his growth and the instrumentation of it by a rationally directed desire for improvement."[4] It is to be earnestly hoped that the revived interest in the study of evolution, as reflected in the new international journal *Evolution,* with its distinguished editorial board, will hasten the psychologist's understanding of causality and increase his sensitivity to the necessity of extending enormously the time range in which he usually considers causality. A better understanding of man's characteristic pattern of growth, rooted in chromosome activities, will, no doubt, also reveal in more detail why, in addition to characteristics peculiar to human beings as such, each particular human being (identical twins excepted), has the unique qualities reflected in his particular capacities and in his temperament.

The individual, in acting to carry out his pattern of growth, both as a human being and as a particular human being, learns particular ways of acting which are comparatively standardized, statistical averages used by others for interpretation. He learns the possibilities available and the consequences of acting in different ways—punishment and reward, acceptance and rejection, etc. If the socially approved possibilities help him carry out his purposes, then these socially approved possibilities become *his* purposes. Those attitudes which have been accepted as a means of satisfying purposes define the individual's status, his loyalties, etc. If the socially approved possibilities do not satisfy an individual's purposes, he becomes maladjusted, or he seeks other means of satisfying his purposes, possibly deviant means such as gangs or membership in an emerging revolutionary group.

Anyone who has been brought up in the school of rationalism, who believes that reason is superior to perception as a guide to action, must have been wondering for some time if, when, and how the role of rational thought and abstractions was to enter into this discussion of perception. The picture would certainly be most incomplete if we failed to indicate, although necessarily briefly, how rational thinking, logic, or acquired intellectual knowledge affects perception.

The precise function of knowledge or reasoning may perhaps be better understood if we review the up-to-the-now ideas or abstractions most laymen have concerning the nature of perception. First of all, we are apt to believe that our knowledge discloses to us what something is and how we can act concerning it; that is, we believe that our actions are determined by our knowledge. Second, we may tend to believe that our perceptions disclose to us what something is and how we can act concerning it; that is, we tend to think that seeing is believing. Third, we may tend to think that knowledge and perception are identical and that we can make the same use of them. However, the psychologist knows and the Ames demonstrations dramatically show that our actions are determined primarily by our perceptions and not by our knowledge. A social psychologist has ample evidence that information, as such, often has very little effect in determining the *direction* of opinion and action.

During the fraction of a second in which most perceptions occur, the number of factors that must be taken into account by an individual are so numerous and so varied that they cannot possibly be considered on a "logical" or "rational" basis in consciousness. I have already referred to the weighing process an individual goes through by the term "value-judgment." This process is largely unconscious, often described as "intuitive." It is easy to observe from one's own introspection and from observation of others, that no successful person bases his life on pure logic or reason—on stepping from one logical conclusion to another. Those people distinguished for their success at any time or in any field are also distinguished for their ability to make value-judgments, not simply for their I.Q.'s and their conscious rational thinking.

It would, however, be the grossest error to conclude that reasoning or knowledge has nothing to do with perception. For the tools characteristic of man's mentality, such as his ability to use concepts and abstractions, although in themselves not sufficient guides for future actions, can, nevertheless, enormously help man carry out his purposes. We need only look at the abstraction of time or the abstraction of tridimensionality to realize how much more efficiently modern man can run his complex life by using them to aid his orientation. Although it appears that we react in terms of our perceptions and not in terms of our knowledge or our conscious logical thought processes to most of the concrete situations we meet in everyday life, knowledge and reason are nevertheless powerful tools to help us get a clearer idea of the true nature of things. Knowledge seems to be particularly indispensable in providing man with the possibility of understanding why his actions have failed to accomplish his purposes. Knowledge, reasoning, and logic enable man consciously to bring more and more cues into the process of value-judgment, thus increasing the reliability of his prognosis.

But it appears that the reliability of our perception as prognostic indicators for purposeful action can only be tested finally by concrete action that we undertake ourselves. True understanding, as opposed to knowledge, seems to be the result of action which we have carried out for a purpose and which checks a perception. Understanding must never be confused with knowledge. Our knowledge helps to guide our actions accurately only in so far as we can make functional use of it. It is obvious that no matter how much knowledge you may have of a subject—whether it is psychology, politics, or anatomy—or how talented you may be, successful purposeful action requires experience and practice on your part. As we have indicated earlier, action gives us a sense of surety, a sense of surety achieved only after we have successfully met frustrating situations. Hence, a person can have no real wisdom in a field where he has no concrete experience. When the disciples asked Christ why he spoke in parables, his answer was "Because they seeing see not; and hearing they hear not, neither do they understand."

Social Perception. After this exceedingly long introduction, we may finally be in a better position to consider the topic of social perception. I should like to repeat what was pointed out earlier, namely, that because a distinction is made between perception and social perception, there is no implication whatever of any inherent differences between the two. I use the word, distinction, in the dictionary sense of the term as "giving special recognition to" certain factors that seem to me to enter into social perception. But there is no "going beyond" any psychological principles that do not apply to the simplest visual perception.

The characteristic factor in a perception we may label "social" is that the functional activity giving rise to the stimulus has a potentiality of affecting our purposes and being affected by us. This potentiality is, of course, our own awareness. And this means that we may impute purposes to stimuli which may have no demonstrable basis, as is the case in animistic thinking. A hornet, a man, a political ideology, may be stimuli for "social perceptions" since they may do something to us or for us and we may do something to them or for them. On the other hand, lightning may be considered a "non-social perception" for it can do something to us while we can do nothing to it. A pencil can be regarded as a "non-social perception" because we can do something to it while it cannot itself do anything to us. If we are not aware of the potential cross-effects of our purposes and the purposes of other organisms, we have no "social perception" in this sense. For example, our intellectual awareness of some distant African tribe, a knowledge we may have of its economic and social structure may be for us a non-social perception as contrasted to our awareness of a European country.

In other words, the functional activity that gives rise to what we may label a "social" perception appears to us to have an important characteristic or "attribute" not part of a "non-social" perception. The "content" of perceptions we may call "social" is likely to be of vastly more significance and importance to us than the content of most non-social perceptions, *i.e.*, color, size, etc. If this formulation of the problem had been made in Külpe's time, I suspect we would be somewhat further along in our understanding of social behavior and might have been spared the artificial division that has so often separated "experimental" from "social" psychology and has been reflected from time to time in a mutual lack of appreciation of the problems and research that constitute the other fellow's area.

This distinction between perception and social perception is the only one that seems to me now to make sense. And it does seem worth while to make a distinction because the implications arising from the added factor of a two-way relationship in a social perception are important for our understanding of man's social behavior. What we may label a "social" perception generally has much greater complexity than a non-social one because of the new factor, *viz.*, the potential purposes of something else, that must be taken into account in the weighing process.

It should be noted that a social perception, as used here, has nothing to do with the inherent (atomic) nature of the source of the stimulus which may be a person, a cow, a symbol, etc. Nevertheless, for most of us, what we can label our "social" perceptions involves other people whose purposes have a potential influence on our purposes. Hence, these perceptions are especially characterized by affective or emotional overtones.

Any perception where the source of the stimulus consists of other individuals is likely to be a "social perception" as used here because of the potentiality which the purposes or actions of those other people have for affecting the carrying-out of our purposes, and the possibility of our purposes affecting the carrying-out of theirs. Other people may be judged in terms of such purposes as: Is he a rival for my girl? Will he take my job away? Will he help me get ahead? Will association with him raise my status or lower it?—etc. These are specific purposes, all of which derive from, are rooted in, man's characteristic pattern of growth. They are cultural or situational derivatives or expressions of this pattern of growth with its distinctive human psychological characteristics. But whatever the environment you have created from the particular stimuli afforded you in your developmental processes, your perceptions as a hunch for purposeful action are given in terms of the possible or probable bearing of concrete stimuli on you, such as the person in front of you, on the carrying-out of your purposes.

Since man's specific purposes are largely derived from the group loyalties and the social norms he acquires, as pointed out earlier, what are "social perceptions" for some people are not "social perceptions" for others. For example, those who believed in animism, perceived objects and forces of nature as things with purposes of their own which could affect human purposes. Anthropologists and sociologists have accumulated ample evidence of the different ways people see things in different cultural groups. And just as lightning was something Benjamin Franklin perceived in a different way than did others of his day, so certain things now believed by some people to be directed to the furtherance or frustration of their personal motives (*e.g.*, "acts of God") may lose their personal reference as man's understanding of nature and of human nature increases. The change in many of our perceptions since the advent of evolutionary theory is a case in point. And as new group loyalties are acquired or as loyalties change, the pattern of what we are distinguishing as social perceptions also changes. For example, the adolescent boy who becomes a member of a neighborhood gang, will then see certain other boys or certain adults as threats or aids to his own newly acquired purposes; American bomber pilots during World War II were reported to have lost much of their anti-Negro prejudices after participating in bombing missions during which they were ably protected by Negro fighter escorts.

This means that one reason for the necessity of understanding between human beings is that only through the mutual disclosure of purposes can we increase the reliability of the prognosis of our perceptions for successful individual action in carrying out our own individual purposes. Or, conversely stated, we will be unsuccessful in carrying out our purposes to the extent that we are unaware of the purposes of others with whom we are associated. Thus, we can cooperate with others only to the extent that our actions in carrying out our purposes help them carry out their purposes. This has apparently been recognized by nearly all the prophets. Hence, on the level of action, nearly all prophets have insisted that we do unto others as we would have others do unto us.

An effective process for understanding another's purpose apparently exists in the mutual recognition of the emergent value-qualities that are consciously experienced when two or more persons participate in joint action furthering a mutually recognized common purpose. A possible explanation of the nature of the mutual interpenetration that operates under such conditions may be the mutual disclosure of the mutual similarity of the highly integrated subconscious processes. These are revealed by the correspondence and similarity of emergent value-qualities. These emergent value-qualities are, as was mentioned before, largely products of subconscious processes.

Our capacity to meet and to adjust to other people's purposes is dependent on our ability to understand these purposes of others. We can understand others' purposes in proportion as we refuse to assign abstract motives to people and in proportion as we deal with them in concrete, emerging situations. For this alone can give us more reliable prognoses as we act on our perception of their purposes in concrete situations that test and modify our perception accordingly.

This implies that our understanding of ourselves, as we acquire it from the activity we label "introspection," is dependent, to a large extent, on our conscious understanding of the purposes of others who are affecting or who might affect our own perceptions, behavior, and purposes. If we have acquired certain status by reason of birth, then whole groups of people may be abstracted out as threats to us—such as Negroes for some Southern whites, labor for the industrialist, and the industrialist for labor. Such people or groups become abstractions. We do not distinguish between their members, nor does it even occur to us that we and they share common purposes as human beings.

Man's unique ability to carry on abstract thinking, to use concepts and reasoning, furnish him, as we have pointed out earlier, with the possibility of bringing more and more factors into the process of value-judgment itself. It would seem that it is especially in the area of complicated social perceptions that man has maximum opportunity to use his unique capacity to bring into the weighing process an awareness of the purposes of others who may be affected by his actions. Furthermore, social perception, especially, is not limited by time and space, involving as it so often does poeple not immediately present.

The relative unsureness of our expectancies in social life is apparently due chiefly to the characteristics of social perception—the potentialities of interaction between our purposes and the purposes of others. As we take into consideration not only the potentialities of this interaction, but also the potentialities of the interaction of the purposes of other specific individuals or groups on still other specific individuals or groups, and the potential simultaneous reciprocal effects of our actions on various individuals or groups, the complexities and relative uncertainties often involved in some social perceptions can be seen to increase rapidly.

Because most of man's purposes must be worked out in a social context, it is in the area we have labeled "social perception" that expectancies become particularly important—the expectancies we have concerning our friends, our co-workers, our competitors, the probability of a depression, the potential reward from superiors, etc.

From the point of view of the discussion here, an "ego-involved attitude" is an attitude catalyzed by a stimulus that can either help or hinder us in carrying out our purposes. Loyalties are ego-involved attitudes that give us a high prognosis that the purposes of others will help us carry out our purposes. A loyalty is a co-product of past experience where perceptions have been tested by action with others and have given us a high prognosis that further action with others will help us carry out our purposes, and the awareness that in carrying out our purposes we will be helping others carry out their purposes. Thus we have a high loyalty, generally, to a wife, a friend, certain groups we are members of, certain reference groups composed of persons whose purposes we believe are more or less similar to our purposes. These loyalties are a person's "my" and "our" feelings. We have no loyalty to a person, group, or symbol that does not satisfy our purposes or give us the possibility of emerging value-experience. Hence, in peace-time, in most countries, people have less loyalty to "their country" than during war-time, since there is no clear-cut common purpose to be carried out by action.

Conflict and frustration can and do occur when the learned value (*i.e.*, statistical average acquired from past experience or the abstraction handed down in the past) involved in an attitude is found to conflict with the carrying-out of a purpose by action—such as practising Christianity in a cut-throat competitive business, practising tolerance and preserving status at the same time, etc. And conflict also results when learned values which conflict with each other cannot be integrated into single purposeful action, *i.e.*, Christian virtues *vs.* unquestionable obedience to a commander or boss.

In other words, what we experience as "personal conflicts" are co-products of conflicting attitudes, of different or contradictory prognoses made when different attitudes are used as a base. Surety in social perception results when there is lack of conflict in weighing these loyalties. There is no conflict of attitudes so long as there is a common purpose between individuals. Since an individual is constantly making a choice, weighing loyalties in order to act, if he finds a choice impossible, he may somehow try to prevent the conflict from arising by putting it beyond recall, removing it from consciousness, etc. Here, problems of psychiatry can be tackled.

From the point of view developed here, it would seem that there can be a scientific basis for judging the "goodness," or the "rightness," or the "correctness" of any social perception. A perception can be called "accurate" or "right" if, through action, it has proved highly effective in carrying out a purpose, in checking a prognosis. It is imperative, however, to remember that because of the reciprocal influence of the purposes of one individual (or group) on the purposes of another individual (or group), actions will prove effective in the long run only when the purposes common to individuals or individuals as members of groups are included in perception.

Although this is a pragmatic approach, it should be clear that this formulation extends the consideration of what makes for effective action to such an extent that it is by no means identical with the pragmatism of James. "Effectiveness" of the type considered here is based on the emergence of value-quality which takes into account the community of human purposes This completely rules out the effectiveness of a ruthless exploiter of human or natural resources.

The "morality" or "ethical" nature—the correctness or rightness—of any action, then, is to be judged in terms of the degree to which it includes and integrates the purposes and provides for the potential development of those purposes of all other people concerned in the action or possibly affected by it. In this way, the "effectiveness" of an action and its inclusion of a common denominator of relevant purposes become inseparable. Also, from this point of view, the "rightness" or "morality" of action becomes inseparable from its effectiveness. This is essentially the Christian ethic which emphasizes charity, *i.e.*, a consideration of the purposes of others. But this does not imply blind tolerance. It will also be recalled that Christ said "I came not to send peace, but a sword."

Emerging personal goals must be in harmony with emerging social goals while still emerging on the concrete individual level. What I am saying, simply, is that "what is right" means what is right for man's nature. Man alone has the intellectual capacity to understand this and to use his intellect as an aid in making value-judgments. And man alone has uncovered the evidence that ceaseless change and emergence is the rule of nature and that evolution is essentially a one-way street, essentially irreversible.[1] It is, therefore, up to man to use his unique ability consciously to plan his affairs, his production, his distribution—his society—so that individual and common purposes will be served and individual and common emergences assured.

This is not merely a moralistic note on which to end. It is a conclusion which seems to me to be based on the evidence science has provided and which follows logically from an understanding of the nature of social perception.

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SECTION OF ANTHROPOLOGY

PONAPE: A MICRONESIAN CULTURE OF THE CAROLINE ISLANDS

By RUPERT I. MURRILL[*]

In the summer of 1947, I was fortunate enough to be a member of the Navy-sponsored C.I.M.A. project. These letters reveal the rather ponderous title 'Co-ordinated Investigation of Micronesian Anthropology.'

It was, perhaps, the largest anthropological expedition ever to go into the field at one time, since at least twenty-one participating Museums or Universities had representatives on the project. The purpose of the latter was two-fold. First, it was a golden opportunity to study the relatively unknown area which had been dominated by the Japanese from the end of the first World War. Secondly, the participating scientists were urged to pass on any useful information they might discover, in order to facilitate relations between the present administrative powers and the natives.

Geographic Description. Ponape is a high basalt island in the Eastern Carolines, approximately 380 miles east of Truk, 900 miles south-east of Guam, 2680 miles from Honolulu and 2360 miles from Manila. The United States Hydrographic Office chart lists Ponape in the Eastern Carolines or Senyavin Islands, the latter name doubtless coming from *'La Seniavine'*, a Russian corvette under the command of Rear-Admiral Lutké who first saw Ponape in 1858.

Tradition says that people first came from the south carrying soil and built Ponape on a reef. Later, additional people came from the west and finally built the structures represented by the present archaeological ruins near Matolenim Harbor in the eastern part of the island.

Ponape is about 130-145 square miles in size, with mountains in the interior rising to 2500 feet. Between the coast line and the foot of the mountains, the land is mostly flat, and it is here that the natives live. It is divided into 5 Districts—Sokas, Net, U, Matolenim, and Kitti. The Districts are divided into Sections and these into farmsteads.

Climate. The climate shows an annual average temperature of 80° F., humidity of 86%, and rain-fall around 180 in. It seems to me that descriptions in the literature of waking up at night in a pool of sweat are rather exaggerated. The humidity is not that excessive. Although rain is liable to fall at any time during the day or night, it soon soaks into the ground, and especially last summer in Districts other than Net the days were mostly clear and sunny. January-March is dry with trade winds, April-June has heavy rain fall, July-September is humid with calms and squalls.

Transportation. This is slow, since roads begun by the Spanish and Germans have never been completed entirely around the island, native paths in the interior are few, and farmsteads are scattered. Thus, one usually travels either by foot, native canoe, or by a dilapidated old Japanese diesel motorboat called a Pom-pom. A 6-8 hour journey on a Pom-pom on a hot day, overcrowded with people, piled high with bananas or breadfruit and towing several canoes, is an unforgettable experience.

Population. My main concern was to make an anthropometric study of the people of the island. In this regard, as I also took their blood pressure and examined their teeth, and as I am rapidly losing my hair, it was amusing to find myself labeled as the 'Wise Doctor.' A man with a stethoscope was obviously a doctor and the natives believe that a man who loses his hair does so because he must 'think hard.'

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[*] Columbia University, New York, N. Y. This lecture, illustrated by lantern slides and motion pictures, was presented at the meeting of the Section on January 26, 1948.

The native population of Ponape, numbering 5,855 as of June, 1947, may be conveniently divided into two main types: (1) 'Pure' Ponapeans—that is to say, those whose ancestry shows no mixture with whites. (2) Mixed. These can be subdivided into: a. Ponapean natives who mixed with whites, such as Americans, English, French, Germans, and Portuguese. b. Ponapean natives who mixed with out-islanders, natives from surrounding islands. c. Ponapean natives who mixed with the Japanese.

As my data have not yet been fully worked out, I am afraid that the above description, though only enough to whet the appetite, will have to suffice for the moment.

Health. Regarding the general health of the natives, the following diseases are commonest: yaws with about 90 per cent infected; tuberculosis 70 per cent; hookworm 100 per cent; gonorrhea 30 per cent of the adult population. These figures may look rather shocking, but the U. S. Navy hospital is doing a splendid job and, in due time, this picture will doubtless be altered.

Resources. Briefly, we can summarize the resources of the island as follows. Yams and breadfruit are the main subsistence crops, with many other cultivated and wild plants being used as food. There are pigs (which take delight in moving in under a house at night and disturbing one's sleep), chickens, goats, carabao, cows, wild deer, and birds. From the sea are obtained many kinds of fish, and the farmsteads and mountain forests, of course, have lumber, flowers, thatching materials, baskets, and other materials for handicrafts.

In the past, exports have included copra, bonito fish, hibiscus bast (kalau), lumber, alcohol from sugar cane, cassava, trepang (sea-cucumber), and tortoiseshell. Furthermore, there was produced for local consumption by the Japanese and natives, paper pulp, cotton from kapok trees, tobacco, rice, liquor, coffee, salt, and many fresh vegetables.

The main town of Colonia in Net District had 4000 Japanese, with restaurants, laundries, a slaughter house, pharmacy, dairy, electric lights, and telephones. However, Colonia was completely destroyed by American incendiary bombs.

The above picture is hardly the typical slumberous South Sea island usually seen in the movies.

Results of Foreign Contacts and Occupations. In the pre-Spanish period of 1826-1886, American whaling boats visited the island, with the usual results of woman-chasing and spread of disease, especially smallpox. The Boston Mission, having christianized Hawaii and looking for greener pastures, cast its eyes westward and finally reached Ponape. It successfully abolished the native religion and most dances.

In the Spanish period of 1886-1899, Colonia was made the main administrative center, roads were begun, Protestant-Catholic clan wars broke out, but more important was the fact that native institutions remained unchanged.

In the German period of 1899-1914, the building of roads continued. In 1910, the German Governor was killed in the 'Sokas rebellion.' Actually, this was another clan war, due to which the Sokas inhabitants were deported to Palau in the Western Carolines and out-islanders were brought in to take their place. Of greater lasting importance were two changes that occurred in native institutions. Whereas, in native times, a commoner held his farmstead as a tenant under the District chief, the Germans now divided the land so that each man owned his farmstead. Also, they proclaimed that land could not be divided by a number of heirs, as was formerly the case.

Under the Japanese, 1914 to 1945, the Sokas inhabitants that had been deported to Palau were returned to Ponape. The German system of land tenure was followed outwardly, but actually conflicting changes occurred. For example, the Japanese allowed land to be divided again among a number of heirs, pro-Japanese heirs were favored, and District land formerly available to commoners for mountain farms was declared Japanese Government property. However, one thing is certain, that during the Japanese occupation the natives were economically at their highest standard of living.

After 1945, we speak of the American period. In the first year little was accomplished beyond returning natives to farms and the establishment of the Navy hospital. However, all District chiefs of higher rank now come together for monthly meetings in Colonia to discuss administrative problems. In short, we have for the first time unification of the Districts.

Building materials have been very scarce, as have been imported goods of all kinds. The reason for the lack of immediate action in this island area is perhaps largely due to the fact that the permanence of America's trusteeship in this area has not been too certain. In addition, other areas such as Japan have doubtless warranted greater attention.

In regard to the visiting anthropologists, the natives were probably somewhat impressed by the fact that a 'pseudo-doctor' was examining them all over the island. They were definitely impressed by the work of the visiting linguist, Dr. Paul Garvin, who settled the vexing problem of differences in the Protestant and Catholic native spelling systems by introducing a new spelling system that satisfied the Protestant, Catholic, and native interests.

Brief Sketch of the Ponapean's Personality. Superficially, the personality of the Ponapean is not easy to assess. I believe the reason for this is largely a historical one. In native times, boys were trained to be strong and brave to endure pain, especially for tattooing and the removal of one testicle which was part of the ritual of becoming a man, and to be a good fighter. Girls were trained to be brave for tattooing and bearing children. Today, the emphasis is to be strong, self-reliant, and a good worker.

In other words, the former overt aggressive pattern has been modified into an aggressiveness, mostly hidden, that is directed toward competition in the new sphere of prestige, that of the ownership of wealth. An economic pattern has replaced the old war pattern. However, in native times and probably to a somewhat lesser extent today, proper deference to higher titled men was and is important on many occasions. Just how much aggressiveness was concealed in native times it is hard to say.

Fitting in with the pattern of deference one finds that politeness is stressed. The form of greeting, also said on departure—*kaseleelya*—is invariably used.

Swearing, practical jokes, openly boasting, are all frowned upon, with the result that one receives an impression of modesty bordering on humility. Compliments may be given but are denied by the recipient. It is interesting that one hears much laughter but usually jokes are at someone else's expense. In a word, humor is malicious.

Shame (*namenek*) seems to me almost an obsession. If a house is not clean or a child urinates on the floor in the presence of visitors, the house-owner is ashamed. Adults are ashamed to instruct boys and girls in sexual matters. There are countless ways in which one can be shamed.

I am not sure how much of this shame is the result of Christianity, as three concepts of 'badness' are stated explicitly. *Me suwet* meaning 'bad,' *tip* meaning 'sin' (Christian), and *into* meaning 'shameful.' The latter are arranged in order of severity, the first being the least severe.

Finally, one is impressed by an over-all atmosphere of quiet dignity.

The Clan. Every Ponapean belongs to one of 22 exogamous matrilineal clans of varying size. These clans regulate marriage, are the basis of hereditary rank, and have economic and ceremonial functions. The clans are widely distributed throughout the island. Each District has certain 'ruling clans' from which chiefs are chosen, but these clans vary from District to District.

Most clans have several sub-clans which are also exogamous. Actually, the distinction between clan (*Tip* or *Sou*) and sub-clan (*Kainek*) were no longer clear to my informants. In the same District, children are taught to love their clan members. Formerly, clan wars were due to clan members aiding each other.

The clan does not help in fishing, farming, and hunting, but does form the basis of co-operative work parties; for example, to help its members build a canoe or erect a house. Clan members of other Districts only help economically by giving hospitality to traveling members.

At the death of a married man or woman, members of both clans come and sit up with the body. A feast is given and if, depending on the rank of the deceased, it is not large enough, the family of the dead person and indirectly the clan are shamed.

Political Organization. Each District is ruled by two parallel sets of 12 main chiefs with up to 10 lesser chiefs. There are Section chiefs and minor titles. In fact, everyone has a title of some sort.

The *Nanmarki* and *Naniken* are the titles of the heads of the two parallel sets of chiefs. Formerly, the *Naniken* was the administrator of all District affairs, the one who 'spoke.' The *Nanmarki* was too elevated to rule or 'speak.' Interesting parallels with Polynesia to be found on Ponape are this 'talking chief,' *kava* drinking, and emphasis on rank.

The Germans made the *Nanmarki* appear in public and manage District affairs. The Japanese made the *Nanmarki* District administrator and the *Naniken* District judge. The Americans continue to deal mostly with the *Nanmarki* although, for example, in Net District the *Naniken* is not only the most intelligent native on the island, but the most influential one in that District.

Formerly, chiefs succeeded one another progressively. If one designated the *Nanmarki* by X_1, he would be succeeded by chief X_2, X_2 by X_3, and so on, but the *Nanmarki* and *Naniken* could and did promote certain chiefs irrespective of their position in the line of succession. This progressive pattern has been modified since there are chiefs who cannot move higher because they do not belong to the correct sub-clan, also certain pro-Japanese chiefs obtained a more rapid promotion than was normally possible.

Each District still builds and owns a District meeting house and feast house. Each District is divided into a varying number of Sections, each of which has a Section chief and feast house, and each Section is divided into farmsteads occupied in turn by a household.

Ponape has no villages, since houses are separated by farmsteads. The Section takes the place of village life. However, certain out-islanders do live in villages such as Mokil village in Sokas and Greenwich village (natives from Kapingamarangi) in Net.

Rank. Three main types of rank are discernible. First is the nobility. These are persons both of whose parents are noble, and such men can become *Nanmarki* or *Naniken.* Secondly, *Seriso* are those who have only one noble parent. They are the children of marriages between nobles and commoners. Thirdly, commoners are those having no noble blood.

Actually, commoners could achieve higher status by becoming stewards to chiefs, or by marrying a noble, or becoming a Section chief. An individual's position then depends on his hereditary and achieved status.

Competition. Yams and breadfruit, although subsistence crops, are also part of a prestige economy that is independent of the commercial economy in which the primary item is the coconut.

By contributing yams to a feast (*kamatip*), a man can obtain prestige. A Section chief will keep an eye on the man contributing the largest yam, as the size of the yam counts, not the quantity, and perhaps reward him with a title. Hence, it is not surprising that yam growing is secret and so many varieties of yams exist—at least 156.

Pit breadfruit, made by ageing breadfruit in a large pit for several years, is next to yams in this prestige economy. In this case, age is important, not size. Pigs and *kava* are an indispensable part of a large feast and are always served according to rank.

A thrifty man, then, is one who deprives himself in order to present yams, pigs, etc., at a feast, yet has more left at home. A wasteful man is one who eats his own food rather than deprive himself to have plenty for a feast.

In conclusion, a new system of prestige based on wealth, as evidenced by the number of coconut trees owned, money, a new house, dishes, good clothes, has recently become in many cases more important than the older form of prestige economy. This new wealth-prestige, however, is part and parcel of the commercial economy, as it provides a stimulus for wage-earning and the production of copra from coconuts.

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Note. The Section of Physics and Chemistry held a meeting on January 6, 1948, at which Professor Serge A. Korff, New York University, New York, N. Y., presented a paper entitled, *Recent Developments on Cosmic Rays,* illustrated by lantern slides and motion pictures. No abstract of this has been received.

THE NEW YORK ACADEMY OF SCIENCES

announces

TWO PRIZE CONTESTS FOR 1948

The New York Academy of Sciences announces the following prize contests for the coming year.

I. THE A. CRESSY MORRISON PRIZE CONTEST

Two prizes of $200 each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Meeting, December, 1948, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

II. THE GEORGE FREDERICK KUNZ PRIZE CONTEST

A prize of $200, offered in accordance with the bequest of the late Doctor George Frederick Kunz, to be known as the George Frederick Kunz Prize in Geology and Mineralogy, will be awarded at the Annual Meeting, December, 1948, for the most acceptable paper in the field of Geology and/or Mineralogy.

Conditions Governing These Prizes

(1) *Eligibility.* Authors and co-authors competing for prizes in these contests shall be members in good standing of The New York Academy of Sciences or one of its Affiliated Societies, but non-members may become eligible by joining one of these organizations before the closing date.

(2) *Date.* Papers are to be submitted on or prior to October 15, 1948, to the Executive Director of The New York Academy of Sciences, at The American Museum of Natural History, Central Park West at 79th Street, New York, N. Y.

(3) *Papers.* All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached.

(4) *Awards.* The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

(5) *Publication.* The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1948 by The New York Academy of Sciences," or, "Awarded a George Frederick Kunz Prize in Geology and Mineralogy in 1948 by The New York Academy of Sciences," as the case may be.

Such a statement, in substance, must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

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Elected January 22, 1948

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

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A Note From The Editor

In reestablishing the TRANSACTIONS of The New York Academy of Sciences as Series II in 1938, after an ad interim *period of 41 years, the Council was guided primarily by requests from the membership. The purpose of the new issue was ". . . to cover the proceedings of the stated meetings; various matters of general interest to the members of the Academy; and, especially, to give an opportunity to scientists presenting papers in the Sectional Meetings to publish abstracts of them at the earliest possible date." (From page 1, Volume 1 of the new Series published in November, 1938).*

During the ten years that have elapsed since that time, this Series has progressively expanded in both scope and distribution. It has not only kept pace with the general development of the Academy, but despite increasing production costs, the coverage of scientific papers contained in the TRANSACTIONS has been improved to the point where, instead of merely printing short abstracts, the papers presented at the various Sectional Meetings are, if possible, published in full.

Volume 9 of TRANSACTIONS consisted of 342 pages. With Volume 10, the format has been changed, so that now each page contains about 30 per cent more text matter than was previously possible. In spite of this fact, the pagination of the present volume will probably run close to 300. Hitherto, the illustrations have been limited to line cuts to avoid excessive costs; but since the reports on teropterin presented at the Biology Symposium in December were exceedingly important, it was decided to issue a Special January Number having a color plate and halftones, as well as a cover. The response to this experiment has been more than gratifying.

The Editors are always on the lookout for possible additional service to the membership and have endeavored to follow the Council's instructions to include "various matters of general interest to the members of the the Academy." So far, this has consisted of records of conferences, prize announcements, summaries of the Annual Meetings, and lists of new members. It often happens that, once a member has been elected, there is little opportunity for him to become generally known to the Academy members unless he takes active part in a conference or serves in some other official capacity. However, his activities outside the Academy may be of regional, national and even international importance, and his fellow-members, naturally, would be interested in learning about them.

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For instance, an item in the New York Herald Tribune *(dated February 11) caught the Editorial attention. Lord Inverchapel, the British Ambassador to this country, recently awarded decorations and medals to certain American citizens who contributed to the Allied war effort in various fields of scientific research and development. The list of recipients of the King's Medal included the names of two of the Academy's Councilors, Doctor Hans T. Clarke and Doctor Caryl P. Haskins, among others of the country's leading scientists.*

This is news of interest to the Academy members, and such items could readily be brought to their attention in the TRANSACTIONS. News of appointments, honors, awards, publication of unusually significant papers, current or projected expeditions can easily be distinguished as desirable foci of such a policy, and the Editors would be glad to print such items; but the Editorial Office cannot properly cover such information without assistance and, in this connection, individual members of the Academy may be of invaluable service to the membership at large by helping us in collecting material of this kind.

It is hoped that there will be an enthusiastic response to this request. The Editors, of course, reserve the right to select such items as would be suitable, so far as space will permit. Clippings and other information should show the name and date of the source publication, if any. Address all correspondence to the Editor of the TRANSACTIONS. The deadline for news contributions to each number would be the tenth day of the month during which the issue goes to press.

The success of this project depends on the cooperation of our fellow members.

SECTION OF BIOLOGY[*]

CHANGES ACCOMPANYING INDUCED RESISTANCE OF STAPHYLOCOCCI TO PENICILLIN

By W. DEXTER BELLAMY[†]

The phenomenal success of penicillin therapy in many infections has stimulated great interest in its mode of action, from both a theoretical and practical point of view. The great amount of time, energy, and money spent on the study of the mechanism of penicillin action indicates the general agreement that more intelligent penicillin therapy, as well as a more logical approach to the synthesis of penicillin-like compounds, could result from a more complete knowledge of the mode of action.

A study of the mode of action of antibiotics can be approached from what may be called three levels of structural complexity. The highest of these is a study of the effect of the antibiotic on the living organisms and their response to it. The simplest of these is a study of the functional groups involved, the relative positions of these groups, and other problems of a molecular order. The level of intermediate complexity is the study of the individual enzymes and coenzymes involved in the action of antibiotics. The matter of determining which of the many hundreds of enzyme-catalyzed reactions are involved has no ready solution. The studies of the effect of penicillin on isolated enzyme systems have been unsatisfactory. Concentrations of penicillin thousands of times the bacteriostatic level have been found inactive when tried in this manner. The whole problem of mode of action in the case of penicillin is further complicated by the fact that it is effective only against young growing cells. A possible approach to this problem is a comparison of the properties of penicillin-sensitive and penicillin-resistant variants of the same organism. The changes caused by growth in the presence of penicillin could indicate the site or sites of action of the antibiotic.

This paper will present some preliminary results obtained by a study of penicillin-resistant variants of *Staphylococcus aureus.*

Cavallito *et al.*[1-3] showed that a variety of antibacterial substances could be classified on the basis of their behavior toward thiols. Some react rapidly with a large variety of -SH compounds, others react rapidly only with cysteine or related β-amino alkane thiols, while still others display reactions intermediate between these two extremes. Klimek *et al.*[4] found that a correlation exists between induced resistance and reactivity toward thiols.

Compounds such as penicillin, streptomycin, and the active principle of *Asarum canadense* are examples of compounds which react rapidly only with cysteine and β-amino alkane thiols. *Staphylococcus aureus* can develop marked resistance against these compounds. The resistance to penicillin was increased 60,000 times in sixty serial transfers. The resistance to streptomycin and to the active principle of *Asarum canadense* increased even more rapidly.

Very little resistance could be built up to allyl-2-propene-1-thiol sulfinate, the active principle of *Allium sativum,* or to the active principle of *Arctium minus*—two compounds which are able to react with a wide variety of thiol compounds.

Intermediate resistance was developed

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[*] The Section of Geology and Mineralogy held a meeting on February 2, 1948, at which Mr. Glen F. Brown, Department of Geology, Northwestern University, Evanstown, Illinois, presented a paper entitled, *The Geology of Ground Water in the Nejd, Saudi Arabia,* illustrated by lantern slides. An abstract of this will be published in a later number of TRANSACTIONS.

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to pyocyanine and gliotoxin—compounds which are intermediate in their reactivity toward thiols. Aspergillic acid appears to lack the power to stimulate resistance development by *S. aureus*. This antibiotic also failed to react with any of the thiol compounds tested.

Both Klimek *et al.*[4] and Abraham *et al.*[5] reported that penicillin-resistant staphylococci grew more slowly than did the parent strain. Highly resistant variants were observed to grow almost exclusively at the surface of broth cultures. It was this observation which led us to suggest the loss of anaerobiosis as a possible cause for slower growth of the penicillin-resistant variant. A corollary of this theory would suggest that organisms that cannot grow aerobically would develop little resistance to penicillin.

Experiments were performed to test the above theory, using a penicillin-sensitive *S. aureus* and a penicillin-resistant variant of this culture obtained by serial transfer in increasing concentrations of penicillin. Aerobically, the resistant variant grew about one-half as fast and, at 50 hours, reached about eight-tenths the final turbidity of the sensitive parent culture. Anaerobically, the penicillin-resistant variant did not grow at an appreciable rate.[6]

The attempts to induce penicillin resistance in three organisms that are not able to grow by aerobic processes alone were unsuccessful. The resistance of *Streptococcus faecalis* increased 11 times in 47 transfers, that of *Streptococcus mastitidis* 6 times in 20 transfers and that of *Clostridium welchii*, 10 times in 25 transfers. The evidence strongly suggests that penicillin interferes with one or more essential components in the anaerobic energy mechanism of susceptible organisms.

Staphylococci are frequently characterized by their ability to produce acid from certain carbohydrates, to grow in the presence of 6.5 per cent sodium chloride, and to reduce nitrates. The increase in resistance was found to be accompanied by a progressive loss of these physiological functions until only the production of acid from glucose remained.[7]

Nicotinic acid or nicotinamide and thiamine are the only growth factors normally required for the growth of *S. aureus*. A penicillin-resistant variant was found to require thiamine for growth but not nicotinic acid. Further experiments indicated that this resistant variant synthesized nicotinamide during growth. The amount of nicotinamide synthesized was equal to that produced by *Escherichia coli* and *Bacillus subtilis*, two organisms known to synthesize nicotinamide.

Many strains of *S. aureus* produce penicillinase, a penicillin-destroying enzyme, but *S. aureus* 209 P does not. Several authors[8, 9] have reported that the penicillinase produced by staphylococci was intracellular and was not obtained cell-free. All attempts to demonstrate an active penicillinase in cells of intermediate resistance were unsuccessful. It was found, however, that when the resistance reached approximately 60,000 times the original, a penicillinase was produced. This penicillinase was found to be extracellular and was produced only in the presence of penicillin.

The morphological changes accompanying induced resistance to penicillin have been studied rather extensively. Since Gardener[10] observed that exposure to penicillin was accompanied by swelling of the bacterial cells, many authors have confirmed and extended his observations. Klimek *et al.*[4] have shown that recently derived resistant variants of *S. aureus* 209 P are larger than the parent culture and are gram-positive. When the resistance became equal to 12,500 times the original, however, the cells were found to be smaller gram-negative pleomorphic cocci and bacilli. Reversal to the original gram-positive character was obtained by transferring to a penicillin-free medium. It was later found that 24 serial transfers at a penicillin concentration greater than 1 mg. per ml. produced a variant that retained its gram-negative character as well as its resistance to penicillin. Fifty-four transfers in broth without penicillin have failed to reverse the gram stain or decrease the penicillin resistance.

Growth in a deficient medium consisting of 0.05 per cent yeast extract, 0.5 per cent glucose, 0.5 per cent K_2HPO_4, 1 mg. per cent thiamine, and 1 mg. per cent nicotinic acid will cause a reversion to the original gram-positive cocci as well as a loss of most of the penicillin resistance.

Gale and associates at Cambridge[11-13] have been carrying out an extensive study of amino acid metabolism of bacteria using the specific amino acid decarboxylases for assays. They have found that the gram-positive bacteria have the ability to concentrate free amino acids within their internal environment until the internal concentration is several times that of the external medium. Certain amino acids such as lysine cross the cell membrane by simple diffusion. Others, including glutamic acid and histidine, can enter the cells only if a source of energy such as glucose is also present. Some surface active agents (tyrocidin, cetyl trimethyl ammonium bromide, and aerosol o. t.) release the internal free amino acids. This was interpreted to be due to rupture of the semi-permeable cell membrane. Electron micrographs of tyrocidin-treated cells showed cell wall damage. Penicillin was found to have no effect on the internal free amino acid content of resting cells.

It was found, however,[14] that penicillin prevents the assimilation of glutamic acid by growing cells of *Staphylococcus aureus*. This loss of ability to assimilate glutamic acid precedes the cessation of respiration and the ability to ferment and oxidize glucose. It appears to be simultaneous with the loss of viability.

Using variants of differing penicillin resistance, it was found that there is no relation between the concentration of free glutamic acid within the cell and its sensitivity to penicillin.[15] The external concentration of glutamic acid necessary to produce a 50 per cent saturation of the internal environment, however, increases as the penicillin resistance increases up to a penicillin resistance of 2,000 units per ml. The highly resistant organism developed by Klimek, as well as one developed by Gale and Rodwell,[16] were found to be unable to concentrate free amino acids within the cell. These highly resistant organisms could synthesize all the amino acids necessary for growth from ammonium sulfate as well as the necessary nicotinamide.

The changes listed above indicate that the organism has been forced to undergo fundamental changes in its metabolic mechanism in order to survive and grow in the presence of penicillin. Further study could indicate which of the many changes are the primary sites of penicillin action and which are secondary. These studies could also indicate means of preventing development of resistance to penicillin.

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SECTION OF PSYCHOLOGY

METHODS OF CONTROLLING PSYCHOLOGICAL FRAUDS

By DAEL WOLFLE*

Since the American Psychological Association keeps me too busy dealing with psychologists to allow much time for cultivating the acquaintance of our fraudulent fringes, I am not personally acquainted with very many psychological quacks. I shall not, therefore, attempt to regale the reader with a lot of case histories of the deeds and misdeeds of the quack psychologists. Lee Steiner's book, *Where Do People Take Their Troubles,*[8] has recently done that job excellently and I am sure everyone is familiar with the fraudulent practices by which untrained and frequently unethical persons who call themselves psychologists gain a living without regard for the welfare of the gullible clients who come to them seeking help.

Though, as I said, I am not personally acquainted with many of these fraudulent characters, the APA office is very frequently at work on one or another phase of the problem of attempting to curtail their activities. It is this problem which I propose to discuss here: How can we protect the public against the psychological frauds?

No one knows how many people there are in this country who earn their living by advising clients on their personal and emotional problems. No one knows the total number, but unquestionably most of them are untrained, incompetent, or outright quacks. A recent article in the magazine *This Week*[4] estimated the number of quacks as over 25,000. In contrast, there are probably not more than 3,500 qualified psychological, psychiatric, and vocational counselors in the country. The quacks, therefore, outnumber the legitimate counselors by at least six to one.

While for the United States, as a whole, I can only estimate, for several cities there have been exact surveys of the people who offer their services as psychological consultants. In the April issue of the *American Psychologist,* we expect to publish brief summaries of two of these studies which were made in Cincinnati and Los Angeles, and I am taking advantage here of an editor's advance knowledge of what will appear in his journal by drawing upon these still unpublished manuscripts.

Edward Shoben[7] found that, in Los Angeles, less than ten per cent of those who listed themselves as clinical psychologists had adequate training by present standards. Only a fifth of those surveyed belonged to the American Psychological Association and only a sixth had an M.A. or Ph.D. in psychology. A review of the 40 persons upon whom he secured most data (out of the 88 individuals who listed themselves as clinical psychologists in the classified telephone directory) shows an interesting variety of occupational backgrounds. Eight had been public school teachers and six college teachers, not all of whom, by the way, had taught psychology. Four were previously social workers and two each had been personnel interviewers, vocational counselors, ministers, and engineers. Two were psychologists, one from a court and one from a hospital. One each had been an artist, a farmer, a speech rehabilitation officer, an osteopath, a bank teller, a lawyer, a musician, an auditor, and a plumber. One had had no previous experience and five refused to give their former occupations.

Seventeen of the 40 had Ph.D. degrees, but only 6 were in psychology. Ten more had graduated from college and the other 13 had no legitimate college degrees, unless we accept those they received from the

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* Executive Secretary, American Psychological Association, Washington, D. C.

College of Universal Truth and similar institutions.

Thirty-five of the self-styled clinical psychologists did not give Dr. Shoben any information about themselves. What their backgrounds were like we can only guess, but the 53 from whom he did secure some information contained several whom one would never recommend as consultants on personal emotional problems. There is, for example, a firm of three consultants whose director is an artist. One of his partners was a plumber for 15 years. The other partner was an industrial engineer without a college degree; in 1945 he became interested in hypnosis, read the literature on the topic, and set himself up as a specialist in hypnotic treatment. Now the artist, the plumber, and the practical engineer have joined forces and are offering their services as counselors and psychotherapists.

I have said that only a fifth of the group upon which Shoben secured reasonably adequate information were members of the American Psychological Association. It is, of course, perfectly possible for a qualified counselor to remain outside the APA, and, unfortunately, it is sometimes possible for an incompetent one to become an APA member. But APA membership is one indication of competence; at the very least, it means that the person has had one year of graduate work plus one year of satisfactory experience in psychology. In the absence of any form of certification or licensing in most states, APA membership provides an imperfect but usable criterion of professional acceptability. Its usefulness is increased by the fact that we know that the APA includes some 90 per cent of the people in the United States who are eligible for membership.

Using APA membership, previous occupation, and education as criteria, I would summarize Shoben's Los Angeles findings by saying that at least three-fourths of the group he surveyed are, at best, inadequately trained, or at the worst outright quacks.

In Boston, Lowell Trowbridge[9] interviewed 70 vocational counselors. Only one had a doctor's degree in psychology. Several had masters' degrees, but most of them were in fields other than psychology. Only 5 of the 70 were APA members.

In Cincinnati, Henry David[2] studied the individuals and organizations listed as psychologists in the classified telephone directory. Of six psychologists listed, two were APA members. One of the other four was listed in another section of the directory as a Registered Masseur.

I do not know of any detailed study of the city of New York, but estimates are easy to secure. The classified telephone directory lists 73 individuals and 37 organizations under the heading "Psychologists." Only a third of these individuals were found to be members of the APA. Some of the organizations were perfectly acceptable; many were not. Incidentally, the best ratio was in Brooklyn, where 6 out of 13 psychologists were members of the APA. In the other boroughs the APA members were greatly outnumbered by the non-members.

If we include related classifications, such as "Vocational Guidance," the total number of persons listed in the directory is 111 and the total number of organizations 123. One-fourth of the individuals are members of the APA. Some of the organizations include APA members on their staffs, but as far as I can tell the great majority do not.

I am concerned over this situation because the average citizen has a right to receive competent help in solving his emotional and personal problems just as he has a right to receive competent help in solving his medical and legal problems, but does not have the means of distinguishing the competent from the incompetent, the ethical from the unethical counselor or psychologist.

Frequently, Mr. John Q. Citizen does not know what ails him. He knows that he is in trouble, and that is sometimes about all he knows of his difficulty. Medicine is better established and better advertised than psychology, yet half the people who go to a physician have "functional" rather than organic ailments responsible for their complaints. As one specific example, 48 per cent of patients who consult doctors for gastrointestinal ailments have nothing organically wrong with their gastrointestinal systems. If the ordinary person does not know what kind of medical attention he needs, how can we expect him to know what kind of psychological service he requires?

Lester Guest[3] has recently conducted a study of the public's knowledge of psychologists. He found a vague but moderately satisfactory level of knowledge of what a psychologist does. Sixty-two per cent of the public have a favorable attitude toward psychologists, and less than one per cent consider a psychologist a suitable person to visit to get one's mind read. But 26 per cent believe that psychologists *can* read minds if they want to; and 40 per cent do not know any reason for ever seeing a psychologist. If the average citizen *does* want to find a psychologist, Guest's data show that he is most likely to ask a doctor to recommend one and next most likely to use the telephone directory as a source of information. Sixty per cent believe that a psychologist must be licensed in order to practise and only 18 per cent know that this is not the case.

I have been quoting statistics to prove that the average citizen has little knowledge upon which he can base an intelligent choice of a psychologist to counsel him on his personal problems. The statistics all point to this conclusion, though, in a sense, they are unnecessary. Every clinician and counselor knows that he frequently has calls from people who are utterly confused over what ails them and what kind of service they need. Many of these people take their troubles to quacks and charlatans.

The public needs protection against these quacks. It needs this protection for three reasons. First, because the money spent on quacks is wasted money. The quack's charges are not determined by his competence, but by what he can collect. Shoben found that, in Los Angeles, fees ranged from $3 to $25 an hour with a median of $10 an hour. If we accept the estimate of 25,000 quacks in the country, assume that they charge the median fee found by Shoben, and assume that they work a standard five-day week and see six patients a day, we arrive at a total annual take of $375 million. This is about 10 per cent of the annual cost of medical care in the United States.[5] It is more than three times the sum that was spent in this country, in 1947, for basic research in all of the sciences. Furthermore, my estimate of $375 million for psychological quacks may be conservative. In the article in *This Week*, the annual cost is estimated at over one billion dollars.

Second, protection is needed to allow the troubled citizen to secure legitimate and competent help in solving his problems. As long as he cannot distinguish the quack from the honest counselor, he has a fair chance of failing to secure the help he needs. This means that, in the extreme case, protection is needed against the danger of outright harm to the troubled person. In many cases his visit to a quack may be harmless, may be nothing more than wasted time and money. But in some cases, his failure to secure competent help means that a minor difficulty grows more serious, a condition that could be remedied is allowed to progress until it is beyond help. The quack's counsel thus may be positively dangerous or harmful.

The public, then, needs protection against wasted money. It needs an opportunity to secure legitimate assistance. It needs protection against the danger of outright harm. These are the reasons for considering how we can drive the quacks out of business.

It is sometimes said that the reason for concern over the quacks in any field is to protect the legitimate persons in that field. This is a useful purpose, but in the field of psychology it is now an unimportant one. Trained psychological consultants have more work than they can handle. If they should have too few clients to consider themselves successful, there are so many other psychological opportunities to turn to that no competent psychologist need be out of work. The only reason worth considering for attempting to protect society against the quack is the welfare of society itself.

There are two groups from whom protection is necessary. They should be distinguished from each other since the appropriate methods of protection differ for the two groups. One includes the well-meaning but inadequately equipped person. The second, usually called frauds or quacks, includes those who are neither honest and well-meaning nor, usually, adequately trained. The occasional person with appropriate academic training and experience who uses those assets unethically belongs to the second group. Actually, of course, the two shade into each other. Adequacy of training is not an all-or-none affair; there are degrees of adequacy and competence. Neither is ethical behavior an all-or-none question; it too shades gradually from the very good to the very bad. For purposes of discussing methods of control and protection, however, it is convenient to separate the persons from whom protection is needed into these two groups.

Protection against the honest but inadequately trained practitioner is almost entirely a problem for the professional associations to solve. The APA, by its membership requirements, the requirements of its Divisions, by its standards and the code of professional ethics which it will some day adopt, and the state psychological associations by similar methods, will be able gradually to eliminate the sincere but marginally trained psychologist. The licensing and certification laws which we shall consider later will also help to remove this kind of psychologist from the scene.

There are four things that we can do to protect society against the psychological racketeer. I shall name them, and the rest of the paper will be spent in their discussion.

(1) We can provide education concerning the problem and can attempt to educate the public to choose its psychological consultants with discrimination.

(2) We can make good services more generally available, so that the public will not have to go to the quack.

(3) We can establish standards of competence that will serve as a guide to psychologists themselves and to members of other professional groups who sometimes refer patients to a psychological consultant. These standards of competence will also be useful to the patient himself in knowing which consultants can be expected to render competent service.

(4) Finally, we can use legal means for prosecuting the quack and attempting to drive him out of the field.

The problem of popular education is one to work on continuously. Lee Steiner's book which I have already mentioned and the reviews and radio programs which it prompted were excellent educational measures, but a great many people never heard of them. Last fall, the Sunday supplement *This Week* published the previously mentioned article[4] describing the extent to which psychological quacks were preying upon the public. Unfortunately, this article overlooked the psychologists and made the statement that the country's 4,500 psychiatrists constituted the public's only protection against the psychological quack. Nevertheless, the article did inform many people of the activities of these quacks.

A more positive kind of popular education is found in a pamphlet written by

Harold Seashore for the YMCA, entitled *All Of Us Have Troubles.*[6] It provides the reader with criteria for distinguishing the quack from the reliable consultant and tells him what to do when he finds himself in emotional difficulties on which he feels the need of help. There should be many more efforts along the lines of Seashore's pamphlet.

Our strongest allies in a campaign of public education are the newspaper and the popular magazine. Last May, one of the Chicago newspapers printed a daily series of articles recounting visits by a reporter to some of the quacks operating in that vicinity. A similar series of articles exposing so-called psychological consultants in Detroit was featured by one of the Detroit papers last September. Here in New York, the newspaper PM is featuring an excellent series of articles written by Albert Deutsch. During the past year, several national magazines have published articles that have served as part of a good educational program. Some have exposed quacks. Some have been warnings that the buyer must beware in buying psychological counsel. Others have consisted of descriptions of the activities of good counseling agencies. All, in one way or another, have helped provide public education on the availability of good and the dangers of bad counseling. Nevertheless, the articles, so far, are only a beginning. As long as the quack psychologist is able to flourish, it will be necessary to continue to provide public information about his activities.

The members of the APA, particularly the college professors, could make an important contribution to public education by spending a little lecture time in describing the community's psychological resources and in giving the name and type of service each agency is equipped to render. Information on how to distinguish a reputable psychologist from a charlatan could profitably be included. Harold Seashore's booklet[6] will provide ideas for such a lecture. Lecture time spent in this fashion would present information to all the class members, and through them to a larger circle of relatives and friends.

The second part of the solution to the quack problem lies in making competent counseling service more generally available. In many parts of the country, such services simply cannot be had. The 1946 *Directory of Psychiatric Clinics* published by the National Committee for Mental Hygiene lists a total of 920 clinics, exclusive of Veterans Administration centers. Of these, 78 are in New York City and 296 are in the State of New York. This is a high percentage for one state. Community clinics exist in only a small number of our cities. School psychologists are unknown in most of the country's schools.

One reason for the scarcity of clinical and counseling facilities is lack of public understanding of their importance. Another is lack of trained personnel. The American Psychological Association has about 5,000 members. Of these 5,000, approximately 1,200 are actively engaged in clinical and counseling work either independently or as part of an organization. The American Psychiatric Association has 4,000 members, of whom 1,100 will see private patients. The National Vocational Guidance Association lists 4,100 members; I have not been able to determine what percentage accepts private clients. Let us assume it is about the same as for the two other associations. If we add together all the psychiatrists, psychologists, and vocational counselors in these three associations, the total is less than 14,000. If we remove from this total the overlapping membership and those who are employed in teaching, research, or administration, or who are in agencies where their services are limited to a specific group, such as the Veterans Administration, a state hospital, or a particular school, the remainder is only about 3,500. Those counselors are available to serve a population of 140 million; that means one counselor to every 40,000 people in the country.

Strenuous efforts are being made to increase the number of psychological and psychiatric counselors available. The Veterans Administration now employs 800 psychiatrists and 300 psychologists. They estimate their real needs, at the moment, as about twice that figure. By 1960, they expect to need 3,400 psychiatrists and 1,500 psychologists. As a start toward meeting these needs, the Veterans Administration now has in training about 400 prospective psychiatrists and about 500 prospective psychologists whose expenses the VA is paying. These students will help relieve the national shortage, but most of them will accept employment with the VA itself and will therefore not be available for general practice.

The United States Public Health Service is beginning to develop the program authorized by the Mental Health Act passed by the 79th Congress. That act authorizes the Public Health Service to provide funds to aid states in the development of their own mental health programs. It also provides money for scholarships to students in training for work in psychology, psychiatry, psychiatric nursing, and psychiatric social work. The Public Health Service program will eventually result in the establishment of hundreds of mental hygiene clinics. Their current goal is 1,300 additional clinics.

This Public Health Service program will go a long way toward making sound psychological counseling services available in all parts of the country to people from all economic and social levels. It is a far-sighted program of social betterment that will remedy many of the existing shortages. By making good services available, it will tend to drive out some of the poor services.

The third way in which the public can be protected against the psychological quack lies in the establishment of professionally and legally recognized standards of competence. Such standards would provide a ready yardstick with which all concerned could measure the competence of anyone who called himself a psychological counselor. We do not yet have such standards. In fact, we have only barely started on the task of establishing them. There are several ways in which such standards can be set.

Educational requirements can be established by a professional association or by an employer. Since the field of psychological counseling is only now emerging into the status of a recognized professional specialty, standards are very different in different agencies and places. They vary from practically no requirements at all in some agencies to possession of a Ph.D. in clinical psychology in others. The United States Army and Navy both require the Ph.D. for commissioning as a clinical psychologist. The Army requires the same degree for commissioning as a personnel psychologist. The Veterans Administration requires the Ph.D. for employment as a clinical psychologist at the P-4 and higher levels. Where strong agencies such as these can enforce high standards of employment, it assures their clients of competent service. It also serves as a pattern to be followed by other agencies. Thus, the Veterans Administration standards and the salary schedule which accompany these standards have forced many state agencies to raise their standards and salaries.

The Army, Navy, and VA requirement of a Ph.D. is higher than the standards set by the American Psychological Association for its members. Nevertheless, membership in the American Psychological Association and, particularly, membership in the Division of Clinical and Abnormal Psychology or in the Division of Counseling and Guidance Psychologists is sometimes used as a test of minimum competence of psychological counselors. But membership in an APA division is not a good method of setting standards. Neither is membership in a state or city association which has higher standards than does the APA. There are so many APA divisions and so many city, state, and regional psychological associations that a psychologist has difficulty in keeping them all in mind. To expect the public to do so is foolish. Thus, to claim as a mark of competence the fact that one belongs to the Metropolitan New York Association of Applied Psychology is not very effective when another and less competent person can honestly say that he belongs to the national one called the American Psychological Association; and still another and this time completely unethical person can claim membership in, let us say, the International Psychological Society or some other non-existent group. Membership in a professional organization can only be useful as a mark of competence if the membership requirements are reasonably high, if they are strictly enforced, and if the existence and meaning of the association is widely known. Psychological associations, generally, are not well enough known to have membership serve as a particularly useful measure of competence.

The meeting of legally established requirements is a much better measure for general use than is the meeting of association membership requirements. Within one state, there can be only one set of legal requirements, while there can be half a dozen competing associations. The public understands the meaning of state licenses or state certificates; it does not understand the membership requirements of most professional organizations. It is, therefore, necessary that we work for the adoption of legal means of distinguishing the legitimate psychological counselor from the quack.

Two types of state laws are possible: certification laws and licensing laws. The reader will be familiar with the merits and difficulties of each, for both have been considered in New York State. Ultimately, we may hope for the general enactment of laws which will license the practice of psychology, much as medical practice is now subject to license laws. But if psychologists are to be licensed in order to practise, the law must contain workable definitions of the practice of psychology.

Satisfactory definitions are difficult to write. In the first place, when a psychologist listens to a client describing his difficulties and then either advises him how to solve those difficulties or lets him work out a solution of his own choice, he is engaging in a practice which is also, and quite legitimately, the province of the family physician, the lawyer, the minister, the social worker, the psychiatrist, and sometimes the old family friend. No licensing law can deny a person the right to seek the advice of these counselors.

If we attempt to define the practice of psychology so precisely that we can distinguish it from the advice given by these other types of counselors, we run into other difficulties. Is the administration of standardized tests a hallmark of psychological counseling? If so, is the school teacher a psychologist? Is the use of a detailed psychological interview a criterion? If so, do we wish to deny to the psychiatrist the right to use interviewing techniques?

It has been suggested that the practice of psychology might be defined in terms of money. Ordinarily, the family friend does not charge for the advice he gives, while the psychological consultant does. However, so does the psychiatrist, the lawyer, and the family doctor. Defining the practice of psychology as psychological service rendered for money is inherently unsatisfactory.

A third possibility is to define the psychologist in terms of the level of knowledge and skill he possesses. If we adopt this definition, we shall distinguish between the psychologist and the non-psychologist by examinations or by the satisfactory completion of a specified course of study. A definition of this type frankly admits what is a fact, *i.e.*, that others besides psychologists give counsel on human problems. But this is also true of the specialized guidance which law and medicine give. Much medical advice is given over the back fence, but a doctor is one who has satisfactorily met the standards required by the state examining board.

Defining the psychologist in terms of his skill and knowledge is, in my judgment, the only workable type of definition. Adopting this standard does not remove the necessity of also defining the psychologist in terms of his activities, but it allows us to specify the activities and then to say that a psychologist is one who performs these activities with a stated degree of proficiency and training. It is along these lines that we must develop our definition. As a start, we might use Armstrong's[1] recent definition of the practice of psychology and proceed to agreement upon the minimum level of proficiency necessary for the right to be called a psychologist.

Until we have arrived at a satisfactory definition, we are not ready to ask for licensing laws. We have not yet reached that stage. The definition of a psychologist is one of the most general and basic problems for the APA to settle. When the definition is written, it will necessarily be somewhat arbitrary. Must a psychologist have a Ph.D.? Must he have an M.A.? Must he have a given amount of practical experience? Can experience be substituted for graduate training, or training for experience? There are no answers to these questions except those that we agree upon. And we must soon agree.

Wherever we establish our minimum standard, we shall of course include a "grandfather clause" in order to avoid injustice to those who are thoroughly competent, but whose formal training was received before the new standards were adopted. After we have agreed and gotten our psychologist defined, we can begin work on the task of persuading state legislatures to enact licensing laws. When we reach that stage, I do not recommend attempting to do the job alone. Psychologists will be licensed, not for the benefit of psychologists, but for the benefit of the society they serve. That fact makes it desirable to have the licensing laws prepared and supported by other groups who are interested in the mental health and general welfare of society. The physician, the psychiatrist, the minister, and the lawyer can help us to prepare legislation that will protect society against the psychological racketeer. If they help prepare that legislation, they will support it; without their support, it will fail of its purpose.

I have been talking of licensing laws of the future. For the present, certification laws are a possible substitute. They do not bar the psychological fraud from business, but they do provide the discriminating client with a criterion he can use in choosing a counselor. Connecticut and Virginia have such laws. It is to be hoped that the one now contemplated in New York is enacted, for it would provide some protection for the citizens of New York and would also serve as an example to other states.

Since the purpose of certification laws is the same as that of licensing laws—the protection of the public against the fraud—it is desirable that they have the support of the other groups interested in community welfare. This means that certification laws, like licensing laws, should be worked out in cooperation with the medical profession and others who are interested in the welfare of society.

The final means at our disposal for protecting the public against the psychological fraud is through legal prosecution. This is a method of very limited usefulness at present. If the fraud, in addition to his purely psychological activities, violates medical practice regulations, there is a legal means of putting him out of business, but it is a risky method, for there is danger that legal action may also harm the legitimate psychologist. The closest cooperation is necessary between the medical and the psychological groups if this method of running the psychological fraud out of business is attempted.

A second legal means is applicable in a few cases. The federal postal laws provide punishment for anyone using the mails to defraud. If a quack psychologist does not conduct his business by mail, he is completely untouched by these laws. If he does use the mail for presumably fraudulent purposes, it is necessary to establish first the fact that he has used the mail. That is normally easy. Second, it must be shown that he has promised to provide certain services or benefits or results if the prospective client will pay in advance for those benefits. This, too, can usually be demonstrated fairly easily. Finally, it must be demonstrated that he has not furnished the results or benefits that he promised. This is more difficult.

So far, only three persons have been charged with using the mails for psychological frauds. I served as a witness for the Post Office Department on all three cases.[10] It is interesting to know that the initiative in all three cases came from the Post Office itself. They had for some time been keeping a number of presumably fraudulent schemes under observation, but had not prepared charges because they had no one upon whom they could call for advice and testimony. Finally, they went to the American Council on Education. There they were referred to the American Psychological Association. They brought me the material they had collected on the three cases, explained what was necessary to secure fraud judgments, and asked for my help. I reviewed the material, helped prepare the charges, and later appeared as a witness for the government when two of the three cases came to trial.

The first of the cases sent, for a few dollars, a set of four lessons that were promised to give the purchaser friends, influence, better jobs, health, freedom from worry, and just about anything else he desired. For good measure, he also received a cheap metal good luck charm. I read the lessons very carefully, but I had no more friends, no more influence, no better job, no better health, and no fewer worries than I had had before. Besides, the post-office inspector kept the good luck charm. This case never came to trial. When presented with charges, its author agreed to go out of business rather than stand trial.

The second case consisted of a set of self-improvement lessons which their author, a mail-order Ph.D., sold under the title *Psychology That Works*. The trial was brief. After evidence showing that he had used the mails to make extravagant promises, and after I had testified that those promises could not be fulfilled by the lessons, the "Doctor" signed a cease and desist affidavit. As we walked out of the court room, the defendant shook hands with me and said, "I was very interested in your testimony, Dr. Wolfle. There was only one point on which I disagreed." His attorney immediately stopped him from saying any more, leaving me still intrigued to learn the one point on which my testimony disturbed him.

The most interesting of the cases was of a man who had advertised that for one dollar he would send a personality questionnaire. Upon receiving the completed questionnaire, he promised to reply with a diagnosis of the individual respondent's personality strengths and weaknesses. Actually, when the questionnaire was mailed in, all that the respondent got was a three-page mimeographed letter and information concerning a home-study course of lessons the man wished to sell and which he promised would result in the advancement, professionally, socially, and economically, of the person who took his correspondence course.

The case well illustrates the difficulty of attacking psychological frauds through the postal regulations. The man was defended by an extremely able trial lawyer who took advantage of every opening given him by the Post Office attorney. Incidentally, he gave me a three-hour cross-examination that I shall long remember. All told, I probably spent five days in helping the Post Office prepare the case and conduct the hearing. Thelma Hunt and the late John Jenkins each devoted a day to it. For defense witnesses, a government personnel man and a psychiatrist appeared. Each testified that the personality analysis furnished in response to the questionnaire was sound and that he would be willing to accept those analyses in his work. Perhaps in their practices they would be justified in doing that. However, the man himself decided that his defense was not strong enough, that the judgment would probably go against him, and that he had better quit. At the end of the second day of hearings, therefore, he promised to cease advertising and mailing his personality questionnaire.

From the standpoint of the Post Office, the trial was a successful one. But from society's standpoint, I am not sure that much was gained. The self-advancement course was not under attack, so that the man is free to continue to advertise and sell it. He will remain so unless the postal authorities decide to bring charges and obtain a judgment against that activity. As for the personality questionnaire and analysis, he is perfectly free to continue to sell it so long as the United States mail is not involved.

All told, the postal laws are not a strong defense against the psychological fraud, since they only apply to persons who use the mails, and even if the trial ends in a judgment against the person, there is nothing to prevent his continuing his activities outside of United States mail channels. Nevertheless, the postal laws provide us with one means of attacking the psychological quack. I have, therefore, asked Dr. Thelma Hunt to serve as a consultant to the Post Office mail-fraud inspectors on psychological schemes that appear to be fraudulent.

The strongest legal means of prosecuting psychological frauds will come when we have established licensing laws. Then it will be possible to accuse them of violating the psychological practices acts and to bring them to trial on those grounds. But first the licensing laws are necessary, and as I have pointed out earlier we do not have these laws and are not yet ready to ask for them. They will, however, come in time. When they do, they will give the public better protection than any method now available.

I have outlined four different methods that may be followed in our efforts to protect the public against the psychological quack: popular education, making good psychological services more generally available, establishment of legal standards of competence, and legal prosecution. Psychologists must work along all four lines. But we cannot work alone. It is in our interest and in the public interest to enlist the help of other groups. Medicine is particularly important, since there is no sharp line between a person's psychological difficulties and his organic ailments—neither is there a sharp line separating the medical quack from the psychological quack. It is to the interest of the public and to the interest of both medicine and psychology to drive out both types of quacks. Medicine and psychology should cooperate in both efforts.

Medical help, however, is not enough. We also need assistance from the clergy, the country's educators, government agencies, and the press. We need, in short, the help of all groups interested in society's welfare. Different groups can help in different ways. The newspapers will be most helpful in public education concerning the problem and concerning the desirability of laws designed to curb the activities of the quack. The other groups can be helpful in general education and in the drafting of appropriate legislation.

What can we expect to accomplish if we follow all four lines of attack that I have described? We certainly cannot expect any great changes overnight. A great deal of work and considerable time will be required. Even then we can expect only partial success, for, despite their greater age and stronger legal position, other professions still have their fraudulent fringes. The shyster lawyer and the quack doctor exist and sometimes thrive despite the efforts of the legal and medical professions to protect the public against their fraudulent activities. We shall never rid the world entirely of psychological quacks as long as some people are avaricious and others gullible.

Thus, we cannot expect complete success. But we can reach a time when the ordinary citizen may know what to expect in the way of psychological service and may know where to find competent ethical psychological consultants. We can reach a time when the troubled person who turns to the classified telephone directory for the name of a psychologist will find the legitimate ones clearly listed. And we can reach a time when the psychological quack will constantly face the threat of prosecution and punishment for his illegal activities.

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SECTION OF ANTHROPOLOGY

THE MOST ANCIENT PERUVIAN FARMERS

By JUNIUS BIRD*

The pictures illustrating this report cover the field work undertaken by the American Museum of Natural History in Peru in 1946-47. This was a part of the cooperative Viru Valley study under the Andean Institute and consisted of an examination of the pre-ceramic midden deposits in the Viru and Chicama valleys.

As virtually nothing is known of the pre-ceramic horizon in Peru, the results of this work will be of some interest, but until the collection is released by the Peruvian government exact identification of the plant remains and technical data on the associated artifacts cannot be determined.

On the basis of field observations, it appears that the huge midden deposits of this early time, one of them running to forty feet in thickness, mark a cultural period in which the local economy was based in part on a simple agriculture, in part on fishing. The associated artifacts comprise a surprisingly meager list: hammerstones, cores, rough unretouched flakes, perforated stone net sinkers, bone bodkins, fish nets, scraps of textiles, mats, basketry, bark cloth, and gourd containers. The cultivated plants were cotton, gourds, squash, chili peppers, four(?) varieties of beans, and *Canna*. None of these are found wild in the valleys today.

Tubers from the "junco" (*Scirpus*) and a nut sedge and cattail roots were also eaten. Of wild fruits, the "lucuma", "ciruela de friarle" and "guava" were the most common.

The balance of food was mainly from the sea: sea urchins, crabs, fish, rarely sea lion and porpoise, and a relatively small amount of shell fish as the adjacent beaches do not produce shell fish in quantity. Bird bones are reasonably abundant but no land mammal bones were found in sifting reasonably large samples of the debris. Cooking was done with hot stones.

The whole complex is more striking by what it lacks rather than what it has. Pottery, maize, weapons, and the pressure technique of flaking stone are outstanding absentees.

At two sites where the debris of the pre-ceramic level merged with debris containing potsherds, the oldest maize was associated with the ceramics of the Cupisnique or coastal Chavin culture.

The lack of pressure flaking is curious, particularly as the same region yielded workshop sites of an apparently earlier hunting people who made many pressure flaked artifacts. Obviously, the farming-fishing population did not here evolve from the hunting stage, for a trait like pressure flaking would not abruptly disappear. The absence of weapons supports this assumption. Though many wood fragments survived, there was no trace of spears, darts or harpoons, nor any points for either group. A few toy sling fragments show the people were familiar with this weapon, but it clearly was of no major importance.

The basketry is all of twined construction, as are most of the mats and textile fragments. The latter are entirely of cotton and show twining, looping, and knotting as the major techniques, with true weaving forming only a minor percentage of the total. A little blue dyed yarn was employed; the only other color, a red, seems to have been limited to a pigment.

Though mud-plastered cane huts seem to have been used to some extent in the early stages, the upper halves of the great pre-ceramic mounds are filled with small subterranean houses. Where cobblestones were available, these were walled up with cobbles set in a mortar made of the mound debris. Lacking cobbles, rough rectangular clay adobes were used to line the same type of structure. With the latest preceramic material are above-ground clay walls and rectangular houses. Accompanying the first pottery are other walls in which cylindrical adobes were set on end. These, in turn, are overlain by walls made of roughly conical adobes associated with debris yielding the Cupisnique sherds. Curiously, the use of stone-lined subterranean houses survived into Cupisnique times. This might be interpreted as confirmation of impressions received at the excavation sites, that the presence of pottery and other distinctive items marks the arrival in the area of a new population which did not drive off or exterminate the established group but blended with it.

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* American Museum of Natural History, New York, N. Y. This lecture, illustrated by lantern slides, was presented at the meeting of the Section of February 28.

As yet we have no positive means of estimating the age of these earliest known Peruvian farmers. Artifact-bearing soil sections show that most of the soil in the valleys has accumulated since the beginning of farming, and this alone implies a considerable antiquity. It does not seem unreasonable to accept the guess date of 1,000 B. C. for the first of the Cupisnique ceramics. If the nature and extent of the earlier debris can be taken as an index, then farming started one to two thousand years earlier.

Note. The Section of Physics and Chemistry held a meeting on February 8, 1948, at which Doctor Turner Alfrey, Jr., presented a paper entitled, *Factors Determining Reactivity of Organic Compounds with Free Radicals (Copolymerization Reaction and Chain Transfer Reactions).* No abstract of this paper has been received.

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Elected February 26, 1948

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SECTION OF GEOLOGY AND MINERALOGY

THE SCOPE OF MODERN SEISMOLOGY

By FRANK NEUMANN*

The scope of seismological interest has expanded enormously since the building of the first modern seismograph in Japan in the early eighties. Following the subsequent discovery that seismic waves traversed all parts of the earth's interior whenever a strong earthquake occurred, astronomers were anxious to check the density of the earth obtained from astronomical data against that obtained from seismological data. Many geologists were keen to know what light such data would shed on the interior structure of the earth, and a few physicists, mostly British and German, set to work to establish the theoretical framework on which much subsequent research was to be based. While activities in these fields marked the birth of modern seismological literature, a highlight of earthquake research not to be ignored was the great catalog of world earthquakes completed by Montessus de Ballore about the turn of the century.

Following the great California earthquake of 1906 and an even greater one the same year in Colombia and Ecuador, world-wide interest in seismology resulted in the establishment of many seismograph stations in all parts of the world and the organization of seismological societies at home and abroad to promote the systematic investigation of seismic phenomena. In the early twenties, the application of seismological principles to the exploration of oil-bearing structure ended a period of relative quiescence. At the same time, the advent of radio communication gave seismologists a badly needed tool for obtaining the required accuracy in measuring the speed of seismic waves. By 1940, about seventy seismological stations were operating in the United States, many of them under the guidance or supervision of central organizations sponsored by either the Federal Government or universities.

From the early seismographs, which often magnified the ground motion only about ten times (fairly adequate for seismic countries like Japan), the magnifying power has gradually been stepped up until now one particular type of instrument magnifies ground motion 100,000 times. This is about all that even a highly favorable site will stand without excessively magnifying the normal background activity caused by wind, microseisms or other disturbances.

With improved instrumentation, modern seismology is now mapping even weak earthquakes thousands of miles from the nearest seismograph station, whether they occur on land or beneath the sea. A

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* U. S. Coast and Geodetic Survey, Washington, D. C. This paper, illustrated by lantern slides, was presented at the meeting of the Section on March 1, 1948.

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million earthquakes, large, small, and very feeble, are now known to occur every year. The great earthquake belts of the earth around the Pacific and across southern Eurasia have been mapped. The energy of earthquakes can now be estimated on a sound scientific basis, and it is found that one great earthquake may expend ninety per cent of all the energy represented in the seismic activity of a year. The discovery that earthquakes may originate as deep as 450 miles below the surface profoundly affected geological thinking as to how such stresses might be set up. The seismograms indicate that shearing stresses are at work at great depths as well as near the surface. It is considered that earthquakes generally represent sudden slipping of rock masses along faults, or the tilting of great crustal blocks, and modern research gives important new clues concerning the nature of these movements.

The structure of the earth as revealed by seismology pictures a great central core, a little more than half the radius of the earth in extent, which reacts to the transmission of seismic waves like a fluid. A crust of variable thickness and complex structure is also indicated. From laboratory tests on rock under high pressure and from geological considerations, it is possible to postulate the types of rock comprising the body of the earth structure.

Further investigation of earthquake phenomena and earth structure require more precise data on seismic wave speeds, especially if the variations in crustal and subcrustal structure are to be adequately clarified. The most important tool of the seismologist is the travel time chart which plots the travel times of many types of seismic waves against the epicenter-to-station distances. More stations and data are needed to outline the true nature of these curves, which reveal the structural details of the transmitting media.

The major unsolved problems of seismology are still the cause and prediction of earthquakes. The immediate causes, such as slipping along faults, block tilting, and volcanoes (for relatively weak shocks) are generally accepted. The explanation of trigger forces such as the attraction of heavenly bodies, earth tides, rainfall and other earthquakes must be accepted with some degree of credibility, but some of these would imply a periodic character to earthquake occurrence which does not exist. The ultimate cause is least understood, but is believed to be associated with the cooling of the earth with resulting shrinking, mountain building, and occasional rupture. The record is not sufficiently complete to explain exactly how erosion and sedimentation come into the picture. There may be slow movements of the semi-plastic magma beneath the crust, possibly slow-moving currents, but the nature of such movements is not clear. Radio activity may play an important role.

It is not possible to predict earthquakes on the basis of their periodicity, because they are not sufficiently periodic. Geodetic surveys which measure the accumulating ground strain prior to an earthquake seem to offer the most hopeful means of earthquake prediction. But definite patterns between ground movements and earthquakes must be developed before such prediction becomes practicable. On the Pacific Coast of the United States, such surveys are a part of the Government's program of earthquake research.

Where an intensive study of local earthquake activity is required, a group of local seismograph stations is necessary. Such groups have been established in central and southern California, Lake Mead, the central Mississippi Valley, along the St. Lawrence River and, to a lesser extent, in other areas. The objective of associating local activity with known fault patterns or other major crustal features has, in some cases, been successfully attained. At Hoover Dam, local shocks were found to be associated with the impounding of great masses of water above the dam. The spotting of active faults obviously has important engineering significance. In the category of regional studies must be included volcanic activity and rockbursts in mines.

The exploration of subterranean structure by controlled explosions and seismic measurement has been developed to a very high degree of efficiency by oil companies. Somewhat similar methods have been employed to study the structure of the continental shelf off the Atlantic seaboard and a portion of the Atlantic Ridge. They have been used by engineers to locate bedrock for the foundations of engineering structures such as dams, bridges, and even roads. In the arctic regions, they are used to measure the thickness of great ice sheets.

In the engineering-seismological field, the ground motions which damage buildings and other structures have been measured with special types of seismographs so that the engineer will know the nature of the earthquake forces to which engineering structures will be subjected. This difficult technical problem calls for special vibration studies and a still different type of instrumentation. The Government, through the U. S. Coast and Geodetic Survey, is taking an active part in such work.

Although the background activity at a seismological station is generally considered detrimental to good earthquake recording, there is a type of activity set up by meteorological conditions which has very practical value. These so-called microseisms, corresponding to ripples on a quiet pond, build up to large amplitudes on the approach of storms, especially at sea. Within the past few years, the Navy has been successful in detecting and tracking hurricanes in the West Indies and is at present extending the project to the western Pacific. Several types of microseisms of unknown origin require further study, but there is little doubt that in some way or other they are related to meterological conditions either near-by or distant.

SECTION OF BIOLOGY

THE BIOLOGICAL SYNTHESIS OF POLYSACCHARIDES

By EDWARD J. HEHRE*

The past ten years have seen the rapid development of the region between biology and chemistry that deals with the natural formation of higher sugars, especially the polysaccharides. For many years, physiologists had been discouraged from experimentation in this region by two erroneous ideas. The first was that the formation of large molecules is an "anabolic" process requiring the uptake of energy, so that the ability seemed likely to be restricted to living things; the second was that substances such as glycogen which are composed of glucose units are formed directly from glucose, possibly by reversal of the process of hydrolysis. However, during the past ten years, cell-free enzymatic synthesis has been achieved for four polysaccharide types, *i.e.*, amyloses, amylopectin- or glycogen-like polysaccharides, dextrans, and levans. For these reactions, no source of energy, apart from that resident in the substrate itself, is required; also, the substrate is not merely the repeating unit of the polymer product.

Enzymatic Synthesis of Polysaccharides of the Starch-Glycogen Class from Glucose-1-Phosphate. In 1937, Cori and Cori[1, 2] showed that the breakdown of glycogen in muscle extracts that takes place in the presence of phosphoric acid yields a non-reducing substance, which these authors isolated and identified as glucose-1-phosphoric acid. Shortly thereafter, Kiessling[3] and the Cori group[4, 5] independently showed that the phosphorolysis of glycogen was reversible, *i.e.*, cell-free phosphorylase obtained from yeast cells or from liver, when added to glucose-1-phosphate, caused the formation of polysaccharide material that had all the properties of glycogen. In 1940, Hanes[6] obtained phosphorylase from potatoes and peas, which brought about the formation of the amylose or unbranched component of starch from glucose-1-phosphate.

Subsequently, the Cori group and others have added a great deal of information on various aspects of the mechanism of phosphorylase action. It was early recognized, for example, that a trace of glycogen or starch was needed to start or "prime" the synthetic reaction, and the synthesis has been pictured as a process of building up upon a small amount of priming material. It is now known that small dextrin molecules may serve as primers, and also that different phosphorylases have different priming requirements. The following equation shows the conversion of glucose-1-phosphate to polysaccharide material of the amylose type, *i.e.*, the unbranched starch component that stains intensely blue-black with iodine:

$$\begin{matrix}\text{glucose-1-}\\ \text{phosphate}\end{matrix} + \begin{matrix}\text{terminal}\\ \text{glucose unit}\\ \text{of primer}\end{matrix} \xrightleftharpoons{\text{phosphorylase}} \text{amylose} + \begin{matrix}\text{inorganic}\\ \text{phosphate}\end{matrix}$$

This reaction is brought about by the phosphorylase obtained from muscle, which tissue normally produces glycogen, and it is also brought about by the phosphorylase obtained from potatoes, which produce a starch that consists of one part of amylose and three parts of the highly branched polysaccharide, amylopectin. Attempts to explain these anomalies led to the discovery of a second enzyme in animal and plant tissue that causes the formation of branches or cross-linkages of the 1:6 variety.[7-9] The formation of amylopectin- or glycogen-like polysaccharides may be expressed by the following equation:

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* Department of Bacteriology and Immunology, Cornell University Medical College, New York, N. Y. The work has been aided since 1944 by grants from the Sugar Research Foundation, Inc. This paper, illustrated by lantern slides, was presented at the meeting of the Section on March 8, 1948.

$$\text{glucose-1-phosphate} + \begin{array}{c}\text{terminal}\\ \text{glucose unit}\\ \text{of primer}\end{array} \xrightleftharpoons[\text{cross-linking enzyme}]{\text{phosphorylase plus}} \begin{array}{c}\text{glycogen or}\\ \text{amylopectin}\end{array} + \begin{array}{c}\text{inorganic}\\ \text{phosphate}\end{array}$$

Certain bacteria also have a phosphorylase system apparently similar to that found in plant and animal tissues. We have recently found [10] that the diphtheria bacillus (*Corynebacterium diphtheriae*) as well as some streptococci will form starch from glucose-1-phosphate but not from other sugars. If it is true, as it now appears, that diphtheria bacilli but not the non-pathogenic diphtheroids produce starch from glucose-phosphate, that property may prove useful in the description and perhaps in the identification of those pathogenic bacteria.

The enzymatic synthesis of polysaccharides of the starch-glycogen class brought to solution a long-standing physiological problem. Because the substrate was a phosphorylated sugar, and because phosphorylated compounds are of such importance in carbohydrate metabolism in general, it was thought by some that phosphorylated sugars would prove to be essential for all polysaccharide synthesis. The following work on the synthesis of dextrans and levans by bacteria and by bacterial enzymes, however, has brought a different aspect to the problem.

Dextrans and Levans and their Formation from Sucrose by Bacteria. Sugar makers have observed the occasional formation of large and troublesome masses of slime or gum in their sugar factories for more than a century. The bacterial cause of this "viscous fermentation" of sucrose, as it came to be called, has been known since the time of Pasteur. Early workers, including the Dutch microbiologist Beijerinck and the Australian bacteriologist Grieg-Smith, recognized that the organisms responsible for slime formation in the sugar factories included *Leuconostoc mesenteroides*, a streptococcus-like bacterium from plants and the soil, as well as various aerobic sporulating bacilli from soil. They also recognized that slime formation occurred only when the bacteria were grown in the presence of sucrose and not when other mono- or disaccharides were substituted for sucrose.

The slimy materials formed from sucrose by those bacteria were long ago found to consist of two polysaccharides: one, a polymer of glucose units, called dextran; the other, a polymer of fructose units, called levan. Dextrans are high polymers of alpha D glucose units linked chiefly through 1:6 positions, but sometimes having branches of 1:6 chains linked to one of the No. 4 carbon atoms[11]; levans are polymers of fructofuranose units, linked in 2:6 positions.[11] Dextrans have a high positive optical rotation; levans are levorotatory. Neither dextrans nor levans are colored by iodine, nor are they hydrolyzed by the amylases that split polysaccharides of the starch-glycogen class. Dextran is more difficult to hydrolyze by acid than is starch; levan is much more readily split than either of these polyglucoses. One of the most interesting biological properties of dextrans and levans is their serological activity.[12, 13] The purified polysaccharides do not, to the best of our knowledge, cause antibody formation when injected into animals,[14] but purified dextrans and levans will give precipitation and complement fixation reactions when mixed with antisera obtained from animals that had been immunized with dextran- or levan-forming bacteria, provided the bacteria used for immunization had been

own in the presence of sucrose. We
ve found[12, 15] that purified dextrans
ve serological cross-reactions with the
a of animals immunized with type 2,
pe 20, and in some instances type 12
eumococci, even though these pneu-
ococci do not form dextran and, when
ed for immunization, are not grown in
e presence of sucrose.

TABLE 1 illustrates the serological reac-
ns of a number of purified dextrans and
ans isolated from sucrose broth cultures
different bacteria, including *Leuconos-*
mesenteroides, Streptococcus bovis,[16]
d *Streptococcus* Lancefield group
[17, 18, 15] all of which are dextran-form-
g varieties; also *Streptococcus sali-*
rius[16] and an aerobic sporulating *Bacil-*
(strain N9), which produce levan.
e dextran-reactive sera include anti-
uconostoc serum and antisera against
eumococcus types 2, 20 and 12. The
an-reactive serum was prepared by
munization with the levan-forming
cillus (strain N9) grown in sucrose
th.[18] All of the dextrans (but neither
an) react in high dilution with the
uconostoc and the *Pneumococcus* types
nd 20 antisera. None reacts with anti-
an serum or with various control sera.
ere is a minor, though definite, sero-
ical difference among dextrans with re-
d to their capacity to react with type
12 *Pneumococcus* serum.[12, 15] Some dextrans, like that from *Leuconostoc* strain A, react as strongly with the type 12 as with the types 2 and 20 antisera; other dextrans, like that from *Leuconostoc* strain B, react only to a slight extent with *Pneumococcus* 12; still other dextrans, derived from streptococci, do not react at all with this serum.[15] Purified levans react with the antilevan serum, but not with the dextran-reactive or control sera.

Particular interest attaches to the formation of dextrans and levans by nonhemolytic or *viridans* streptococci because of the possible application to the differentiation of those bacteria. Levan formation has been utilized in a redefinition of *Streptococcus salivarius,*[19] and the dextran-forming capacities of *Streptococcus bovis*[16] and of streptococci belonging to Lancefield group H[15, 17] may eventually aid in the description of those varieties. Dextran formation is a property also of a variety of streptococci found in the blood of approximately 40 per cent of patients with subacute endocarditis.[15] These streptococci, like dextran- or levan-forming bacteria in general, elaborate large quantities of the polysaccharide from sucrose, which capacity is fundamentally responsible for the success of experiments on the *in vitro* synthesis of dextrans and levans by enzymatic means.

TABLE 1

SEROLOGICAL PROPERTIES OF PURIFIED DEXTRANS AND LEVANS

	Antisera					
rose-derived olysaccharides	*Leuconostoc**	*Pneumococcus,* type 2	*Pneumococcus,* type 20	*Pneumococcus,* type 12	*Bacillus,* sp., *N9**	Controls
trans						
euconostoc, A	++++	++++	++++	++++	0	0
euconostoc, B	++++	++++	++++	+	0	0
trep. bovis	++++	++++	++++	0	0	0
trep. group *H*	++++	++++	++++	0	0	0
ans						
trep. salivarius	0	0	0	0	++++	0
acillus, sp., *N9*	0	0	0	0	++++	0

Bacteria used for immunization grown in media containing sucrose.

TABLE 2

SEROLOGICAL COMPARISON OF PRODUCTS FORMED FROM SUCROSE BY STERILE ENZYME SOLUTIONS WITH PRODUCTS FORMED IN LIVING CULTURES

Strain of *Leuconostoc*	Reactivity *vs.* types 2, 20, and 12 anti-*Pneumococcus* sera			
	Enzyme-sucrose mixtures		Sucrose broth culture fluids	
	2 and 20	12	2 and 20	12
B	++++	+	++++	+
M	+++	0	++++	+
O	++++	+	++++	+
A	+++	+++	+++	+++
C	++++	++++	++++	++++
K	+++	+++	++++	++++

++++, precipitation with 1 5000 or 1 10000 dilution of the antigens
+++, precipitation with 1 500 or 1 1000 dilution of the antigens
+, precipitation with 1 10 or 1 20 dilution of the antigens
0, no precipitation with 1 10 dilution of the antigens

Synthesis of Dextrans and Levans by Enzymatic Means. Early workers, in particular Beijerinck,[20] had reported the production of gum-like material from sucrose by filtrates from various species of levan-forming sporulating bacilli. Their products were not, however, chemically or serologically identified, their filtrates not proved to be free of bacteria, and their results and ideas about synthetically active enzymes consequently were forgotten. In 1941, we were able to establish[21, 22] that sterile Barkfeld filtered extracts prepared from sucrose broth cultures of *Leuconostoc mesenteroides* brought about the *in vitro* synthesis of dextran when incubated with sucrose. Purified preparations of the dextran formed by the cell-free enzyme from sucrose were examined and found to be similar, both in serological and in chemical properties, to the dextran elaborated in sucrose broth cultures of the *Leuconostoc* bacteria from which the enzyme was derived. The possession of serological activity by the enzymatically synthesized dextrans is of special biological interest, and represents a feature that can be utilized to particular advantage in examining the question of the likeness between the polysaccharides synthesized by cell-free enzymes and the natural polysaccharides formed in living bacterial cultures.

An illustration of the excellent serological agreement between dextrans formed in enzyme-sucrose mixtures and those formed in living cultures is given in TABLE 2.[23] Sterile enzyme solutions were prepared from sucrose broth cultures of 6 strains of *Leuconostoc mesenteroides,* representing two serologically different types. The enzyme solutions were incubated with sucrose, and the mixtures then compared serologically with the supernatant fluids obtained from sucrose broth cultures of the 6 *Leuconostoc* strains.

The dextrans produced by the sterile enzyme-sucrose mixtures, like those from the bacterial cultures, reacted not only with antiserum of *Leuconostoc* but also cross-reacted with the antisera of types 2, 20 and 12 pneumococci. From TABLE 3, it is apparent that the enzyme-sucrose mixtures of 3 strains (B,M,O), like the culture fluids of those strains, had only slight capacity to react with type 12 in comparison with their capacity to react with types 2 and 20 *Pneumococcus* antisera; whereas the enzyme-sucrose mixtures of the remaining 3 strains (A,C,K), like the culture fluids of those particular strains, had as high a capacity to react with type 12 as with the types 2 and 20 antisera. Since the comparison was made by a test capable of detecting minor serological differences, these results indicate the very close serological likeness between the material formed from sucrose by the cell-free enzymes and the material formed in living cultures of the strain of bacteria from which the enzyme had been derived.

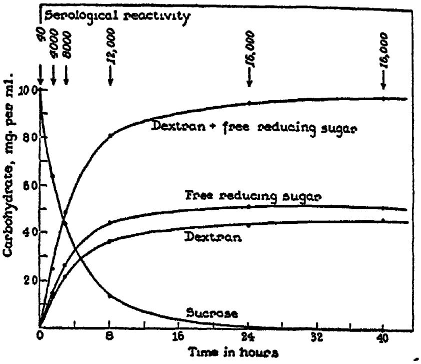

FIGURE 1.

The *Leuconostoc* enzymes were early found to be active upon sucrose and, to a slight extent, upon raffinose (galactosidosucrose), but are without effect upon glucose, invert sugar, or other common sugars.[21] The overall action of a *Leuconostoc* enzyme upon sucrose is illustrated in FIGURE 1. On incubation of enzyme and sucrose, polysaccharide formation is accompanied by the liberation of a reducing substance which has been identified as fructose. In the experiment of FIGURE 1, in which chemical measurements were made on all of the known components of the synthetic reaction, the conversion of sucrose to dextran and reducing sugar began without a noticeable lag period, proceded at a rate that diminished in an orderly way with time, and finally approached completion. Throughout the reaction, a regular relationship between the dextran and the free reducing sugar was produced, associated at all periods with a corresponding loss of sucrose. The serological reactivity of the mixture, shown at the top of FIGURE 1, represents the highest dilution of the mixture that gave precipitation with a dextran reactive antiserum.

The relationships found in this and in many similar experiments suggest that the enzymatic reaction of dextran synthesis from sucrose may be expressed as:

$$\underset{\text{Sucrose}}{n\,C_{12}H_{22}O_{11}} \rightarrow \underset{\text{Dextran}}{(C_6H_{10}O_5)n} + \underset{\text{Fructose}}{n\,C_6H_{12}O_6}$$

The cell-free synthesis of dextran from sucrose by enzymes from *Leuconostoc mesenteroides* was confirmed by Stacey,[24] and the synthesis of levan from sucrose by enzymes from a soil bacterium (*Aerobacter levanicum*) established by Aschner and Hestrin.[25-28] The equation proposed for levan formation is analogous to that for dextran formation, *i.e.*,

$$\underset{\text{Sucrose}}{n\,C_{12}H_{22}O_{11}} \rightarrow \underset{\text{Levan}}{(C_6H_{10}O_5)n} + \underset{\text{Glucose}}{n\,C_6H_{12}O_6}$$

The formation of levan has many parallels with the synthesis of dextran. An outstanding difference is that levan formation proceeds as well from raffinose as from sucrose, whereas dextran formation is slight or absent from raffinose. The kinetic aspects of dextran and levan formation have been presented in several reports.[27-29]

An interesting point, hitherto unreported, is the capacity of *Leuconostoc* enzyme to convert very low concentrations of sucrose to dextran. Because of that capacity, and because it is possible to recognize very small amounts of dextran by serological means, it has been possible to devise a delicate semiquantitative test for sucrose. With it, sucrose can be recognized in as high dilution as one part in one hundred thousand, either alone or in the presence of large quantities of glucose or other sugars.

TABLE 3

SEROLOGICAL REACTIONS OF PRODUCTS SYNTHESIZED FROM SUCROSE AND RAFFINOSE BY THE CELL-FREE ENZYMES

		Highest dilution of enzyme-substrate mixture that gave precipitation in tests with different antisera			
Source of enzyme	Substrate	*Streptococcus salivarius* antiserum	*Bacillus N9* antiserum	Type 2 *Pneumococcus* antiserum	Control sera
Streptococcus salivarius	sucrose	1,000	1,000	5	
	raffinose	400	400	0	0
	other sugars†	10*	10*	0	0
	buffer solution	10*	10*	0	0
Bacillus N9	sucrose	1,000	1,000	0	0
	raffinose	1,000	1,000	0	0
	other sugars†	5*	5*	0	0
	buffer solution	5*	5*	0	0
Leuconostoc mesenteroides	sucrose	0	0	10,000	0
	buffer solution	0	0	10*	0

0 = no precipitation with 1:5 dilution of the antigen.

* Represents reactive material contained in the original enzyme solution, which did not increase in amount during the incubation of the test mixtures

† Glucose, fructose, glucose plus fructose, maltose, lactose, trehalose, xylose or arabinose

We have made experiments to determine whether the products synthesized from sucrose and raffinose by cell-free enzymes from levan-forming species would have serological properties like those of the levans produced in the living cultures. As shown in TABLE 3, cell-free enzymes from *Streptococcus salivarius* and from a sporulating levan-forming bacillus, when incubated with sucrose or raffinose, but not with other common sugars, gave rise to products that precipitated levan-reactive antisera. These products were serologically different from the dextran synthesized by *Leuconostoc* enzyme, shown at the bottom of the table. Some of the enzymatically synthesized polysaccharide was isolated from an incubated sucrose-*Streptococcus* enzyme mixture. It had chemical and serological properties like the levan produced in cultures of that particular strain of *Streptococcus salivarius.*

While equations expressing the overall reactions of synthesis of dextrans and levans from sucrose were obtained from early experiments, the exact mechanism by which these syntheses take place could be established only by further experiments. Analogy with the enzymatic syntheses of glycogen and starch raised the question of the participation of phosphorylated intermediates in dextran and levan formation. The possibility that glucose-1-phosphate might be a required intermediate in the conversion of sucrose to dextran was especially significant. Dextran is not too far removed structurally from polysaccharides of the starch-glycogen class and, furthermore, it was known that several kinds of bacteria, including *Leuconostoc mesenteroides,* can under certain conditions convert sucrose into glucose-1-phosphate and fructose. Doudoroff and his colleagues[30] showed that the reaction is reversible, and demonstrated the first biological synthesis of sucrose from glucose-1-phosphate and fructose through the action of "phosphorylase" from the bacterium *Pseudomonas saccharophila.*

In order to get data on the question of the role of phosphorus in dextran formation, a comparison was made[31] between *Leuconostoc* enzyme and potato phosphorylase in respect to their action upon sucrose and upon glucose-1-phosphate. Polysaccharide-synthesizing enzyme from a *Leuconostoc* culture and from potatoes was incubated with 0.1 molar solutions of sucrose and of glucose-1-phosphate buffered at pH 5.6; control mixtures of the enzymes in buffer were included. After 4 hours, all the test mixtures were examined for evidence of polysaccharide formation (TABLE 4). The *Leuconostoc* enzyme, when incubated with sucrose, gave rise to abundant dextran formation with liberation of free fructose; the potato phosphorylase, when incubated with glucose-1-phosphate, caused the formation of abundant amounts of starch with the liberation of inorganic phosphate. In contrast, the *Leuconostoc* enzyme had no demonstrable action upon glucose-1-phosphate and the potato phospherylase had none upon sucrose. The lack of reactivity with glucose-1-phosphate on the part of the *Leuconostoc* enzyme, which was so reactive with the sucrose, can be taken as evidence that glucose-1-phosphate is not an intermediate substrate in the enzymatic synthesis of dextran from sucrose. The absence of any appreciable amount of phosphate in the system in which abundant dextran was formed suggests that the enzymatic synthesis of that polysaccharide does not require the mediation of any phosphorylated sugar substrate but occurs by a direct condensation of the sucrose in the way indicated by the equation given above. Evidence presented by Hestrin *et al.*[27] indicated that the enzymatic synthesis of levan from sucrose also does not require phosphorylated sugar intermediates.

TABLE 4

COMPARISON OF *Leuconostoc* ENZYME AND POTATO PHOSPHORYLASE IN RESPECT TO THEIR ACTION UPON SUCROSE AND UPON GLUCOSE-1-PHOSPHATE

Source of enzyme	Substrate	Properties of test mixtures incubated 4 hrs, 25°C					
		Appearance	Ppt with alcohol	Serological reactivity	Color with iodine	Reducing sugar, mg./ml.	Inorganic phosphate, mg /ml.
Leuconostoc	sucrose	opalescent	++++	20,000	yellow	6.2	<0.001*
	glucose-l-phosphate	clear	0	40	yellow	0.1	0.03
	buffer control	clear	0	40	yellow	0.1	<0.001*
Potato	sucrose	clear	±	0	yellow	0.3	0.07
	glucose-l-phosphate	cloudy	++++	0	blue-black	0.3	6.4
	buffer control	clear	±	0	yellow	0.3	0.07

* No phosphate was detected by the Fiske-SubbaRow procedure or by the Denigès method, either before or after the mixture had been digested with sulfuric and nitric acids

Our conclusion in 1943[31] was that the synthesis of dextran by *Leuconostoc* enzyme and the syntheses of starch and glycogen by the phosphorylases, though superficially different, nevertheless have a fundamental point of similarity. That is, in each instance, the substrate (sucrose or glucose-1-phosphate) contains the basic unit of the final polymer product in the form of a glycoside radical that is exceedingly easily split off by acids. The same relationship obtains in the enzymatic synthesis of levan, because here also the basic unit (fructose anhydride) is contained as a glycoside radical in the substrates (sucrose or raffinose) from which the levan is derived.

The activity of glucose-1-phosphate as a polysaccharide precursor thus would appear to depend upon structural features possessed also by the sucrose molecule, and not its being a phosphate "ester" as is commonly believed.* From that point of view, in contrast to the general assumption (see, for example, Cori [32] and Hassid, Doudoroff, and Barker [33]) that all polysaccharides of the starch-glycogen class arise from glucose-1-phosphate, sucrose might well be a precursor of starch or glycogen. An example of the possession of this capacity by sucrose was obtained early in 1946 when we found that certain gram-negative bacteria from the healthy throat do convert that disaccharide into a glycogen- or amylopectin-like polysaccharide.[34, 35]

Formation of Amylopectin-like Material from Sucrose by Neisseria *from the Pharynx.* The bacteria which have the capacity of forming amylopectin-like material from sucrose fall into the genus *Neisseria perflava,* and form a prominent part of the normal flora of the pharynx and nasopharynx. When they are grown on media containing sucrose, iodine-coloring material is produced; when grown on media containing common sugars other than sucrose, no such material is produced. The polysaccharide can be obtained in amounts as large as 3 to 5 grams per liter from cultures grown in 5 per cent sucrose broth. Chemical analyses show an amylopectin-like nature, clearly distinguishable from dextran. The polysaccharide is soluble in cold water to give colloidal solutions that are intensely opalescent and that stain maroon or purple-red with iodine. Both these properties are rapidly lost on treatment with saliva.

Data on the mechanism of the polysaccharide production were obtained in experiments such as the one shown in TABLE 5. When washed bacterial cells from glucose broth cultures were incubated with 0.05 molar solutions of various sugars in maleate buffer at pH 6.4, abundant amounts of polysaccharide were formed in the case of the test mixture which contained sucrose. Separate analyses were made on the sediments and supernatant fluids of the mixtures. The sucrose yielded considerable amounts of iodine-coloring polysaccharide in both the sediment and supernatant fluid fractions; glucose-1-phosphate yielded some but much less polysaccharide than the sucrose; and none of the other sugars tested (*i.e.,* glucose, glucose plus fructose, lactose, maltose, trehalose, α-methylglucoside, melibiose, raffinose or melezitose) yielded any demonstrable amount of polysaccharide.

TABLE 5

PROPERTIES OF INCUBATED MIXTURES OF *Neisseria* CELLS WITH VARIOUS SUBSTRATES

Substrate	Sediment		Supernatant fluid					
	Dark color with I_2	Polysaccharide, mg./ml.	Opalescence	Precipitate with 1 5 vols. alcohol	Dark color with I_2	Polysaccharide, mg /ml.	Free reducing sugars mg./ml.	Inorganic P, mg /ml.
Sucrose	++++	1.48	+++	+++	+++	0.61	2.75*	<0.01†
Glucose-1-phosphate	+	0.13	0	±	0	0 08	0.05	0.04
Other sugars‡	0	0.05	0	±	0	<0.06	—	<0 01†

† The total acid-soluble P was less than 0 05 mg per ml

‡ Glucose, glucose + fructose, lactose, maltose, trehalose, α-methylglucoside, raffinose, melibiose, melezitose

The data obtained in this experiment with washed bacterial cells furnished better evidence for the substrate specificity of the polysaccharide synthesis than could be obtained by experiments with growing cultures, because the concentration of the bacterial agent and the pH were constant in all the test mixtures. The pronounced preference for sucrose distinguishes the polysaccharide formation by the neisseriae from the production of iodine-coloring reserve carbohydrates which has long been known to occur with various bacteria and fungi.

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* On the basis of physico-chemical considerations alone, Wolfrom and Pletcher[38] concluded in 1941 that glucose-1-phosphate resembles an acetal or glycosidic structure more closely than it does a true ester. Nevertheless, in spite of biological and chemical evidence, the terms Cori ester, 1-ester, ester bond, etc continue to be used in connection with this substance.

In the sucrose test mixture, in which abundant polysaccharide formation had occurred, there was no accumulation of inorganic phosphorus, and the total phosphorus concentration was very low. These points, together with the much greater yield of polysaccharide from sucrose in comparison to that from glucose-1-phosphate suggest that, when sucrose is the substrate, glucose-1-phosphate is not required as an intermediate substance. Decisive evidence was obtained in other experiments conducted in the presence of high concentrations of inorganic phosphate. In test mixtures in which the molecular concentration of inorganic phosphate was increased to 6 or 8 times that of the substrate, the formation of polysaccharide from glucose-1-phosphate was entirely suppressed, whereas the formation from sucrose was not inhibited.

In regard to the mechanism of the action upon sucrose, the points to be emphasized from TABLE 5 are: first, the free reducing sugar liberated into the fluid is fructose, as shown by the strongly positive Ekkert test; and, secondly, the amount of the reducing sugar corresponded reasonably well with the amount of polysaccharide in the fluid plus that in the sediment. These features suggest that the synthesis of amylopectin-like polysaccharide by the neisseriae is the same general type of reaction as the synthesis of dextran by *Leuconostoc* enzymes: that is, *n* molecules of sucrose are converted into *n* molecules of fructose plus a polysaccharide molecule containing *n* glucose anhydride units. It is of interest to speculate on whether or not similar conversions of sucrose to polysaccharides of the starch class may not have importance in plant metabolism.

Work is now in progress with a cell-free enzyme system, isolated from cultures of these *Neisseria,* which is capable of converting sucrose to glycogen- or amylopectin-like material. We have named this enzyme system *amylosucrase*. That term indicates the relationship to Hestrin's *levansucrase* and to the dextran-synthesizing enzyme or *dextransucrase*. It also emphasizes the distinction from the phosphorylase-plus-"cross-linking enzyme" systems obtained from plant and animal tissues, which convert glucose-1-phosphate to amylopectin or glycogen. In spite of the distinction, however, the synthesis of polysaccharides of the starch-glycogen class both by the neisseriae and by the plant and animal phosphorylases are fundamentally similar. The substrate (sucrose or glucose-1-phosphate) in each instance contains the basic unit of the final polymer product in the form of a glucoside radical that is exceedingly easily split off from the parent compound by acids but not by alkali. Phosphate is involved apparently in only one of the reactions and may not even be essential there. According to very recent work of Katz, Hassid and Doudoroff,[36] arsenate can replace phosphate in the breakdown of starch by potato phosphorylase. Glucose-1-arsenate, the expected product, has proved so labile that hydrolysis has so far prevented its isolation.

The ever widening study of reactions of polysaccharide formation is leading to increased understanding of other aspects of carbohydrate biochemistry. For example, our studies and those of Hestrin *et al.* have established the fact that sucrose enters reactions in which its glucoside and fructoside units are transferred, apparently without the mediation of phosphorylated compounds, to polysaccharides of several types (dextran, levan, amylopectin-like material). More recently, Doudoroff, Barker, and Hassid[37] have shown that the glucoside unit of sucrose also can be transferred to several disaccharides. All these studies, made with bacteria or with bacterial enzymes, show that sucrose is a versatile donor of its glycoside groups. If similar glycoside transfer reactions involving sucrose occur in plant or in animal tissues, we may be on the way toward understanding the role of one of the key sugars in nature.

What seemed ten years ago to be the end of a long-standing physiological problem (how glycogen is broken down and formed in the body) has proved to be but the beginning of a wide and rapidly advancing front in the field of biological reactions of carbohydrates.

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SECTION OF PSYCHOLOGY

THE SAMPLING PROBLEM IN PSYCHOLOGICAL RESEARCH

By HELEN M. WALKER*

Although a large proportion of the research studies in psychology involve the generalization from sample to universe, so that the procedures for drawing samples correctly is a problem of central importance in research work in psychology, and although in the last decade other fields such as biology, industry, agriculture, market research, opinion research, and governmental surveys have made very notable developments in the procedures for drawing samples scientifically, nevertheless there has been relatively little reference to these developments in psychological literature and one gains the impression that too few psychologists are aware of them. Unless the sample is drawn at random from a known universe or drawn at random within segments of that universe, or unless the observations can be assumed to be a random sample of a hypothetical universe, there is no way at all of drawing correct inferences from that sample to a larger population.

Empirical data were presented to illustrate the meaning of sampling distribution, the advantage of stratified random sampling, and the fallacies inherent in the ordinary use of pre-existent groups. Descriptions were given of the methods employed by the U. S. Census Bureau for obtaining a master sample of agriculture and a sample survey of unemployment.

The following implications for sampling in psychology may be noted:

(1) The overall size of a sample does not alone determine the sampling reliability of its statistics. By using a good design, a smaller sample can often be made to produce greater precision. A sample may be very large and still furnish no measure of precision and practically no reliable information.

(2) Sampling has become a highly technical business with a very large literature which cannot be safely ignored by the research worker.

(3) Certain kinds of studies, especially of the large survey type, are now almost out of bounds so far as individual research goes. A large-scale successful survey needs long preparation, a field staff, financial support, and a certain amount of highly technical information about sampling.

(4) In any survey, a random sample of refusals should be carefully followed up.

(5) Pre-existent groups should be used with the greatest caution. A preliminary survey might be made to see whether several such groups differ reliably as to the trait under consideration.

(6) In large-scale surveys, more use might be made of the punched cards on communities which can be obtained from the U. S. Bureau of Census.

(7) There is urgent need of a nationwide master sample of persons which could be used in psychological, sociological, and educational studies of human beings. The promotion of such a sample would be an appropriate undertaking for the American Psychological Association.

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SECTION OF ANTHROPOLOGY

A SURVEY OF NEAR-EASTERN ANTHROPOLOGY

By RAPHAEL PATAI*

The application to the Near East of those sciences which are called anthropological came at a relatively late date. The primary interest which first focused scientific attention on the Near East was that in the Bible, its background, historical and geographical setting, its archaeology and linguistic affinities. Accordingly, the whole area came to be designated and known by the name, "Bible Lands," and its history came to be looked upon from the viewpoint of what light it could shed upon the "sacred history" recorded in the Bible. All the countries which were the setting for any of the happenings related in the Bible were "Bible Lands," and the newly awakened interest, based upon the unquestioning belief in the "accuracy" of the Bible, proved to be a powerful stimulus for the study of these lands. The very specific archaeological investigation and a general sort of exploration of the present conditions prevailing in "Bible Lands" developed hand in hand.

The purpose of archaeological work was to unearth contemporary evidence as to Biblical history, while in the general exploration the emphasis was on the light thrown on the Bible by a study of the geography, natural conditions, fauna and flora of the "Bible Lands," and by the "manners and customs" of their present-day inhabitants. The results of these investigations soon found their way into Bible commentaries, and, due to them perhaps as much as owing to the textual and "higher" criticism of the Bible, a new and better understanding of the Holy Book was reached.

Scientific interest, once aroused, tends to continue, if not to increase. The studies begun for the Bible's sake were continued for their own sake and soon developed into separate sciences, each with a rapidly increasing body of knowledge of its own and with a corresponding tendency to specialization. Archaeology was the first to grow up into a full-fledged science in its own right, and excavations on a scale unprecedented even in Greece were soon being conducted in the "Bible Lands," with techniques more and more refined and methods of drawing conclusions more and more precise. The occurrences of successive layers in archaeological sites in Palestine and Mesopotamia became documents of utmost significance, the deciphering and interpretation of which with ever increasing accuracy enabled the science to trace back the beginnings of history in this region to the fifth millennium B. C.—that is, to a period by far preceding not only the first epochs of Bible history but even the traditionally accepted date of Biblical creation itself.

With the deciphering of the Egyptian hieroglyphics and the cuneiform scripts of ancient Mesopotamia, two new sciences were born, Egyptology and Assyriology, each of which today is an important and complex science in its own right. Beginning with a groping interpretation of inscriptional material which was brought to light, first, through chance findings and later through systematic excavations, the study of the languages and of material objects unearthed grew to complement each other more and more until a clear picture could be gained of the material culture and the political history of the respective countries, as well as of their religion, arts, sciences, literature, ideas: in brief, the culture of these countries, the peoples of which rose already in the very dawn of history to amazing heights of material as well as spiritual achievement.

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In another direction, the interest in the language of the Bible led to the study of the tongues related to it. Affinities with Babylonian and Assyrian, and even with Egyptian, were discovered and stressed, and the study of Arabic, Aramaic and other languages belonging to the Semitic family was pursued in addition to the study of Hebrew itself. The comparative study of the classical Semitic languages has reached a level equaled only by that of the Indo-European languages and, lately, written remains of entirely new languages have been discovered belonging both to the Semitic family, such as the language of the Ras Shamra-Ugarit tablets, and to non-Semitic languages such as that of the Hittites.

The disciplines referred to above are made use of by history which, drawing upon all of them, has by now succeeded in giving a fairly continuous picture of what happened in these lands of the East during a period some four times the length of European history. In all, it can be said that the studies pursued with real zeal and energy in the Near East for the last two or three generations have been historically oriented, a fact due to the very nature of this area in which culturally poor and apparently decadent peoples live in the shadow of magnificent ruins eloquently testifying to a long and glorious past.

We come now to survey the developments in the second trend of investigations which originated even earlier than the archaeological study of the "Bible Lands," namely, the general exploration of the present conditions prevailing in these countries. We have to state at the very outset that, if we expect to find here anything like the well-founded and systematically collected body of knowledge characteristic of the historical disciplines, we shall be keenly disappointed. For one reason or another, the study of the present conditions of the Near Eastern area has largely remained in the "Bible Lands" stage. Therefore, when speaking about the various fields of anthropological study in the present-day Near East, I shall have to dwell more often on problems which have yet to be solved, on tasks which have yet to be fulfilled, on fields that have yet to be explored, on approaches which have yet to be utilized—in brief, on what has yet to be done, rather than on what has already been done and achieved.

Let us begin with a brief and necessarily sketchy survey of the linguistic study of the modern Near East. A glance at the study of modern Arabic dialects will be sufficient to characterize the difference in the grade and extent of our knowledge of the classical Semitic languages on the one hand and of their modern derivatives on the other. Every aspect of classical Arabic, the language of the Koran, has long ago been thoroughly explored, so that our knowledge of its vocabulary, its grammar, linguistic affinities, and the like, leaves little if anything to be desired. Among the numerous modern Arabic dialects, on the other hand, there is not a single one which has been studied with any thoroughness even approximating this, while many of them are veritable *terra incognita* to this day. The domains of the Arabic dialects extend today, in addition to Arabia proper, over Iraq, Syria, the Lebanon, Palestine, Transjordan, Egypt, Tripolitania, Tunis, Algeria, and Morocco. The idioms spoken are so different that the inhabitants of different countries often cannot understand one another. Moreover, within one single country itself, various dialects exist, in accordance with either the geographical divisions of the country or the social stratification of its inhabitants, or both. In Palestine, small as it is, we have a good example of this double dialectic variegation: there is a marked difference between the language of the southern and the northern part of the country, and then, again, a still more marked difference between the dialects of the urban Arabs, of the settled villagers, the *fellahin*, and the nomadic *bedouin*. A systematic survey of all the local Arabic dialects would be highly desirable, while, at a later stage, based on a survey of this kind, a comparative study of modern Arabic dialects could and should be made, somewhat on the lines of the famous *"Vergleichende Grammatik der semitischen Sprachen"* of Karl Brockelmann.

Arabic has influenced, in the Near East as well as in regions peripheral to it, a number of other languages, such as Aramaic, of which we shall hear more anon, Persian, and the Sudanic languages. Here, again, there is a field for the study of linguistic processes taking place when groups speaking different languages come in contact, and in the course of which one or both of them adopt certain elements from the language of the other.

While Arabic greatly expanded in the course of the last millennium, Aramaic, which in ancient days played the role of an international language in the whole Near East, lost its ground and is spoken today only by a few small and isolated groups. Apart from a few villages in the Lebanon, Aramaic is spoken only by the Nestorian Christians and by the Jews of Kurdistan. These so called New-Aramaic or Syriac dialects have been studied by Prym and Socin in Tur Abdin, and are being studied at present by my friend Dr. Joseph J. Rivlin among the Kurdish Jews. Comparative studies of the various New-Aramaic and Old-Aramaic dialects are still lacking.

As to Hebrew itself, it, too, has its new branches. I am referring not only to the revival of Hebrew in Jewish Palestine, where it has become, in the course of a single generation, a complete modern language capable of serving as a wholly adequate vehicle of modern literature, philosophy and science alike. Apart from this modern, and in part artificially, or rather consciously created Palestinian Hebrew, there is the modern survival of traditional Hebrew in the Jewish communities of Near Eastern lands, such as the Hebrew spoken by the Jews of Hadramaut, the very existence of whom became known only a short time ago.

We shall have to skip the Semitic languages spoken outside the Near East proper, such as the various Semitic dialects of Abyssinia, as well as the non-Semitic languages of the Near East itself, like the various Persian and Turkish dialects and only say, in conclusion of our brief presentation of the linguistic aspect of Near Eastern anthropology, that the vernaculars belonging to a number of major and minor groups have yet to be mapped out and investigated—a task which will require much patient and painstaking labor.

Before turning to the study of the Near Eastern peoples themselves, I should like to say a few words about the state of the geographical exploration of the area, for, of course, an adequate geographical survey is one of the prerequisites of any ethnological study. Our geographical knowledge of the Near East varies with its different subareas. On the one hand, there are countries such as Palestine, which have been surveyed as accurately and in as detailed a manner as any European country, so that on its large-scale map sheets one can find every smallest well or *tell*; while on the other hand there are large areas never yet visited by an outsider and represented on the map by a blank space. It was only a few years ago that Bertram Thomas traversed, on camel-back, one of these blank spaces in the *Rub' al-Khali,* the great South Arabian desert. As a result of his crossing, we now know that along a single route of some 700 miles connecting Dhufar on the southeastern shores of the Arabian peninsula with Qatar on its northeastern coast there are, for instance, some fifty wells, or rather waterholes, the existence of which alone makes human and animal life possible in the *Ar-Rimal,* The Sands, as the Empty Quarter is called by its own people. Thomas also supplied valuable information as to the flora, the fauna, and the peoples along the route he took, but many more such crossings in all directions will be necessary before we can pronounce the geography of Southern Arabia even passably well known.

Leaving aside, of necessity, the more or less unknown areas of the Near East, let us now turn to the physical anthropology of its peoples. Again, we must preface our remarks with a warning as to the inadequacy of the available information. Anthropometrical measurements have been done only sporadically, and even where executed the individuals measured numbered only ten, twenty, or at the utmost forty. It is on such scanty factual material that the current classifications of the peoples of the Near East are based.

Taking physical anthropology in its widest sense, we should begin with a reference to the remains of fossil or prehistoric man in the area under survey. Since, however, these finds are very few in number and nowhere connect with unmistakable directness and clearness with the inhabitants of the area in historical times, I do not propose to say anything about the results arrived at or the problems arising from a study of human palaeontology in the Near East, but shall proceed instead directly to a brief discussion of the physical types of the inhabitants of our area in proto-historic and historical times.

To take head form first, it was found that the inhabitants of ancient and modern Mesopotamia, of ancient and modern Egypt, and of Northern Arabia were and are long-headed. The peoples inhabiting what can be called the northern belt of the Near East, stretching from Afghanistan and the Pamir in the east to Syria and Turkey in the west, as well as the peoples of Southern Arabia, as far as can be judged from the few measurements made, are decidedly round-headed. As one goes southwards in Syria, round-headedness gradually decreases, its place being taken by long-headedness. In the areas of the occurrence of both types in Syria, it is the *fellahin* and the urban population which are round-headed, while the *bedouin* are long-headed similarly to the bedouins all over the northern half of the Arabian peninsula. As head form, that is, the cephalic index, is taken to be one of the basic characteristics of races, the occurrence of these two different types in the Near Eastern area would alone be sufficient to make us aware of the fact that it was and is inhabited by peoples showing at least two different physical types. The two types, however, show a number of additional differences. The northern belt, stretching, as already mentioned, from Afghanistan and the Pamir westwards to Turkey, is the home of the so-called Armenoid type which is characterized, in addition to brachycephalism, by a flat occiput, lofty head, long face with a prominent aquiline nose. This type, or one very similar to it, occurs also among the South Arabs, who, however, according to Keith and Krogman,[1] are only half Caucasian, the other half of their features pointing decidedly to Hamitic (or Dravidian) affinities. Among these traits let us mention the very dark, almost black complexion, black fuzzy hair, chin tufts, small body, and certain facial features which are reminiscent of the peoples of the neighboring parts of Africa (Somali, Danakil, Hadendoa) and the Egyptians throughout nearly eight millennia.

The inhabitants of the southern shores of Arabia (the Yemen, the Sheher, and Muscat) form, as it were, connecting links between the round-headed South Arabs on the one hand and both the long-headed North Arabs and the long-headed Hamites on the other.

The North Arabs, occupying the territory between the South Arabs and the northern belt referred to above, are distinctly Semitic-Caucasian. The nomads inhabiting Mesopotamia, Syria, Palestine, Transjordan, and the northern half of the Arabian peninsula are long-headed. In stature, they are taller than the southerners. In pigmentation, it is only in northern

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[1] See Appendix to B. Thomas. Arabia Felix: 319. New York. Scribner's. 1932.

Syria that a small proportion of blonds appears, though in general the north is lighter brown than the south.

In North Africa, the Berbers, mostly sedentary and agricultural, were the ancient inhabitants and to this day are the majority. They are of medium stature, very light brown in skin color, and brunet in hair and eye color. Interesting enough is the presence of a blond element, constituting in some places up to 12 per cent Head form, though showing considerable variations, seems to be dolichocephalic in the fundamental type. On this substratum, the Arab type has been imposed following the Mohammedan conquest in the 12th and 13th centuries.

From the foregoing, it will be clear enough that, when speaking of the Near East, I am referring to the countries of the North African belt, to the Arabian peninsula and the territories adjoining to the north and the east of it. I think I should mention that it is by no means generally accepted to designate these lands, precisely, by the name "Near East." You will often find that the western part of North Africa is excluded. The term "Near East" itself is frequently discarded for the newer term "Middle East," which became fashionable in the course of World War II, and which is often understood to comprise an even wider area than the one indicated above. Seeing that there are such differences of opinion as to the extent of the area, in addition to the variety in the physical types of the peoples inhabiting it and in the languages spoken by them, I think it would be timely to explain what considerations seem to justify our treating this great expanse of lands as one single unit, and in what respects traits making for unity can be discerned in it.

Let us take a marked natural characteristic first. This entire area is subdivided into two very distinct geographical types: a region which is mediterranean in character with long, hot, and rainless summers, rainy, temperate winters, and a natural vegetation ranging from grass to open deciduous forests; and a region of deserts with great extremes of temperature, almost no rain at any season, and a very scanty vegetation of low grass and drought-resistant bushes. The dividing lines between the desert and the cultivated land, or the "sown," as it will be hereafter referred to, which runs along the entire Near Eastern area, must of course not be imagined to be anything like a clear-cut boundary. As a rule, there is a semi-desert or steppe belt of varying width between the true desert and the true sown, and, owing to the presence of this belt, parts of which can be swallowed up by either side, the borderline between the desert and the sown is neither definite nor constant, but is liable to shift forward or backward yielding to prolonged droughts or to abundant rains, and equally to population pressure exerted on it by the nomads of the desert or the agriculturists of the sown.

With the mention of the nomads and the agriculturists, we have reached a second general characteristic of the entire Near Eastern area: just as each Near Eastern country comprises both desert and sown, so its population is composed of both nomadic and settled elements. The desert has been for uncounted centuries, one may even say, millennia, the home of various types of pastoral nomads. The common characteristics of nomadic life all over the Near East, as far as can be generalized from the very inadequate information available, are: animal husbandry with a reliance mainly on the camel as a means of transportation, source of food (camel milk), supply of raw material for hair-tents, clothes, leather vessels and trappings; seasonal wandering with the herd within a certain territory, in the dry season near the steppe and the settled land, in the rainy season far out into the heart of the desert; tribal organization with a sheik at its head, collective responsibility expressed in such institutions as the blood-feud, raiding and hospitality (the law of sanctuary);

and a preference for the desert with its precarious but free life and a contemptuous attitude towards the "sown" with its relatively greater security coupled with the compulsion of unceasing, toilsome labor. The *bedouin* have a great familiarity with nature, the sands, the sky, the stars, the flora and the fauna, and show both in their physique and in their mentality a remarkable degree of adaptation to the unparalleled harshness of desert life.

These traits, to be sure, are found in a pronounced form only among the true nomads, that is, the camel-bedouins, who actually spend a great part of the year far away in the desert, cut off from any contact with settled people; who often have to traverse great distances over waterless sands or stones in search of pastures, or wells, or herds, or in pursuit of or retreat from an enemy. Among what we may call the half-bedouins, whose main possession are not camels but sheep and goats, one finds only rudiments of these characteristics. Owing to the inability of the flock to cover great distances, these semi-nomads have to remain near the settled lands even in the rainy season, mostly within the steppe-belt or the fringes of the desert which turn green after the first rains. Their limited mobility, their closer contact with agriculturists, the very difference between tending camel-herds and sheep or goat flocks, all these resulted in the semi-bedouins' taking, as it were, an intermediate position between the true nomads and the settled population.

To visualize the relative importance of the desert and the sown on the one hand and of the settled and the nomadic population on the other, it will be well to mention here that, while the cultivated land can be estimated as constituting about 10 per cent of the whole area of the Near East, it supports about 90 per cent of the entire population of the area. The demographic picture of the Near East thus shows a large, fairly dense agricultural settled population occupying a small percentage of the entire area on the one hand, and on the other a small minority of pastoral nomads scattered over the vast expanses of the desert.

Now, as to the settled population of the Near Eastern area, it again is composed of various elements. There are, first of all, the agriculturists, the *fellahin,* or peasants, living in village communities largely similar to one another all over the Near East, though of course showing local variations in conformity with the geographical and climatic conditions, as well as with religious and ethnic differentiation. A village in the mountains of Kurdistan is very dissimilar to one on the banks of the Upper Nile which, again, will be unlike one along the Moroccan coast. Likewise, a Christian village in the Lebanon, a Moslem village in Syria, and a Druz village in the Jebel Druz will each have its own peculiar characteristics not shared with the others. The building material of the Near Eastern village is stone in the mountains, mud in the plains, reed in the marshes, and palm-leaves in the deep south. The stone houses, for instance, are everywhere one or, rarely, two-storied buildings with few and small windows, flat or vaulted roofs; they are surrounded by dusty or muddy backyards with low dividing fences. The village well is mostly outside the village and often at a lower level. The presence of a village makes itself felt from a distance owing to the acrid smell of burnt dung. In contrast to the *bedouin* camp in which the tents are pitched at a fair distance from one another, the village is a compact and tightly packed cluster of houses or huts, with narrow, uneven, and winding passages of communication left open between them in the most haphazard manner. Surrounding the village are its lands, extensively cultivated and, apart from the neighborhood of the great rivers, such as the Nile, but rarely artificially irrigated.

The inhabitants of the village are *fellahin,* cultivators. But while in a bedouin tribe each and every family subsists on animal husbandry, the villages have, in addition to their bulk of agriculturists, also a few artisans, such as a cobbler, a blacksmith, a weaver or a mason, as well as other specialists such as a teacher, some other religious persons, and the like. Social stratification goes hand in hand with occupational specialization, so that in most villages one finds landowners, tenants, and hired hands, each occupying quite a definite rung of his own in the scale of social positions.

In the *bedouin* tribe, the families of which traditionally trace back their descent to a common, often eponymous or mythical ancestor, every man is equal; the power vested in the sheik of the tribe is yielded to him voluntarily and he is expected to act always with an eye to the interests of the tribe as a whole. Ownership of property does not play a considerable role in the determination of personal status—wealth, reckoned in heads of camels, can quickly dwindle in consequence of a few seasons of drought, or even more suddenly disappear in the wake of resourceful enemy raiders; or, again, relative poverty can within a few days turn into wealth in consequence of a single lucky raid. What determines, therefore, the status of a man are his personal courage and valor, or, in later age, wisdom in counsel and, the greatest blessing of all, a large number of sons.

In the settled population, the situation is quite different. Here, the village is the social unit, the families of which may or may not be related to one another. The social status of the individual is strongly influenced by his wealth, reckoned in land and money. The head of the wealthiest family is often recognized by the village *de facto,* and by the government *de jure,* as head of the village. As such he divides, even in the best case, his efforts between three aims: the furtherance of his own wealth, of that of the village, and of the interests of the government. In many cases, especially in Palestine, Syria, and the Lebanon, the land of entire villages is the property of absentee landlords, to whom the *fellah* has to pay rent in crop or cash.

To give a general characterization of Near Eastern village life is a much more difficult task than in the case of the nomads, which we have attempted above. First of all, there is the marked difference between the pronounced heterogeneity of the villagers as against the relative homogeneity of the nomads. The desert is largely the same throughout the entire Near Eastern area, and so are its sons, the nomadic camel-herders. The villagers, on the other hand, are as different from each other as are their villages. The inhabitants of villages perched amid the wild mountains of Kurdistan with their rough climate are rugged, often ferocious people, given to hard drinking, forced to fight nature constantly and inclined to fight each other on every occasion. The villages of the Kurdish mountain tracts are often impenetrable natural strongholds, independent of any outer rule, subject even at the utmost only very nominally to the government of the country to which they are reckoned to belong politically. A Kurdish village is a self-relying and self-sufficient unit socially, politically, and economically alike. They are, in fact, settled tribal bases, and as such much nearer in many respects to the nomadic tribes of the desert than to the *fellahin* of the villages bordering upon the desert.

How different from this proud, unbridled, and self-willed folk are the *fellahin* living in the villages of Upper Egypt! Klunzinger,[2] who knew Upper Egypt before the processes of change set in, having lived there in the sixties and the seventies of the last century, says of the *fellah* of that country, "Though belonging to the very lowest rank, he knows how to conduct himself with propriety, almost with polish, in society; he may be often rough, but he is never rude and boorish, a main reason for this being that he never

[2] Klunzinger, C. B. Upper Egypt: 125. London. 1878.

gets drunk. Compliments flow from his eloquent tongue till they verge on flattery and falsehood. He is entertaining, witty, full of fancy, good-humoured in ordinary intercourse if at all well treated; but he is mendacious, deceitful, avaricious, fond of begging, and even thievish, when there is any question of *meum* and *tuum*. Patiently, like the camel, he bears the burden laid upon him; but if it seems too heavy he becomes stubborn and self-willed, until he has received a due number of official blows upon the soles of his feet, whereupon he immediately becomes obedient again and bends slavishly before his Turkish master, his superior in energy."

A third important component of the peoples of the Near East, in addition to the nomads and the villagers, are the inhabitants of the towns. Towns were a characteristic feature of Near Eastern lands ever since the dawn of history. Among the most ancient remains of Near Eastern culture are the ruins of towns having the form of *tells*, that is, mounds composed of several successive layers of ruins testifying to several successive epochs of settlement with periods of desolation intervening. These *tells* are mostly very small; in fact, whenever I visited one, I was again and again astonished at their smallness. They rarely exceed two hundred yards in any direction. If you then ask what is the difference between a village and such a small town, the answer must be that the difference lies not in relative size but in mode of construction: towns in ancient days were, as a rule, fortified, while villages were open and unprotected. Mostly, a number of villages were dependent on each town—it is these villages which in the Bible are called "the daughters" of the town—and in times of danger their inhabitants fled behind the relative safety of the walls and gates of their "mother" town.

Towns with dependent villages surrounding them are characteristic to this day of the settled areas of the Near East, even of its deep interior. St. John Philby, when crossing the Arabian desert, found that the oases of the Nejd comprise a central town with scattered hamlets surrounding it and with a few square miles of cultivated land, the population of such an oasis being sometimes as many as 10,000.

As to the distinction between fortified town and open village, this held good down to Turkish days, and it was only in the nineteenth century that outside the walls of the old cities new, open quarters began to grow up, thus giving rise to the characteristic dual appearance of the main towns of the Near East, comprising a small nucleus of an old, walled-in part and a surrounding open, new city.

The most characteristic part of the old cities is no doubt the *suq*, the bazaar. This is a network of extremely narrow streets, mostly covered with vaulted stone roofs or with matting or awning, and having small, open shops on both sides. Each such street is occupied by a different trade, so that there is a *suq* of cloth-merchants, a *suq* of sandal-makers, one of coppersmiths, of spice vendors, butchers, fruit-sellers, jewelers, and so forth. This arrangement, which has, of course, its counterpart even in such a modern city as New York, gives bargaining an additional pleasure, for the buyer can play up the price demanded by one merchant against another. Shopping in the East is in the main bargaining and haggling, so that purchasing even the smallest and cheapest object can easily become an affair protracted over one or even several days. The *suq* is, moreover, the meeting place of everybody; in it or adjoining it are found the cafés where men sit for hours on low backless seats over a small cup of strong Turkish coffee and a gurgling *narghileh* (water pipe) and which are usually crowded and serve as the clearing place for news and rumors. Another meeting place is in and around the gates leading through the walls into the old city. Outside the gate is the traditional place of the animal market and of the potter displaying his fragile ware, and here meet the *bedouin* and the *fellahin* coming to town to sell or buy. The colorful congregation of all sorts of people in the *suq* and at the gate is the most prominent feature of Near Eastern towns, and, unless one has rubbed shoulders with the throng filling the narrow lanes, one cannot claim to be acquainted with the Orient.

Oriental towns, of course, are much more than a framework for the *suq* and the commercial activities in it. In addition to the merchants and manufacturers, we find living in them members of the few great and rich families who hold in their hands a large share of the economic and political control of the country. The government and its offices are in the towns, and in them live the officials employed in national, regional or local administration. The few academicians and intellectuals included in a traditional oriental society—doctors, lawyers, teachers in higher than elementary schools, writers, many of whom have studied in a foreign school—also have their abode exclusively in the towns, side by side with the more traditional types of intellectuals, such as the preachers and other dignitaries of the mosques, judges of the religious courts, administrators of the *waqf* (charity foundations), leaders of youth and other movements, experts in folk-medicine, soothsayers, diviners, and the like.

Another characteristic of the larger cities all over the Near Eastern area is that in them live strong non-Moslem or non-native minorities with an often higher economic level and having the role of westernizing agents.

A significant feature of the Near Eastern area as a whole is the constant interaction between the nomadic and settled population. This interaction, we know today, has been going on in the Near East since the earliest times. One of its aspects was described in the 14th century by the noted Arab philosopher-historian, Ibn Khaldun, who stated that the population of the sown becomes weakened by its relatively comfortable mode of life so that it cannot repel the attacks of the stronger and more ferocious sons of the desert. The nomads, however, once they have occupied the fertile land, gradually lose their original vigor until they, too, fall victims to the incursion of another group of the desert people.

The same process goes on today, though no more in the form of raids and invasions, but of peaceful, individual or small-group settlement on the part of the *bedouin* on the peripheries of the town. Sir Alec Kirkbride recently described the distinguishable stages of this process in Transjordan, where it is a spontaneous movement. The same process, sponsored by King Ibn Saud, goes on in Arabia proper.

This, however, is only one aspect of the interaction between the desert and the sown. Another, less spectacular, but in the long run perhaps more significant aspect is the economic contact between the *fellah* and the *bedouin*. The mutual opinion of these two is, to be sure, anything but favorable. It is a commonplace which found its way even into Toynbee's books that the nomad regards the agriculturist as a stick-in-the-mud, while the agriculturist regards the nomad as a vagabond. This mutual disdain and distrust notwithstanding, the *fellah* and the *bedouin* are, to a certain extent, in need of each other and frequently meet in marginally situated markets, such as Beersheba in southern Palestine, to buy products of animal husbandry and to sell agricultural or industrial products and *vice versa*. It is an interesting comment on the far-reaching influence of western civilization that, the more advanced the process of westernization in the cultural centers of a country (that is, in its main cities), the greater this economic interaction between the settled and the nomadic population will be.

The same is, of course, true to an even greater extent of the dependence of the settled rural population on the towns. This, too, grows in direct relation to the intensity of westernization. There is, moreover, a trend to move from the village into the towns, where a man can be more independent socially and better off economically on industrial labor than in the village on the extremely low pay of agricultural labor. The difference between the status of an industrial laborer in the towns and that of an agricultural laborer in the villages can best be characterized by mentioning one significant fact, *viz.*, while in many Near Eastern countries industrial laborers today have their recognized organizations, it still remains illegal for agricultural laborers to form unions.

In addition to the contacts between the various parts of the indigenous population, there is, in the towns all over the Near East, a tendency to assimilate to western civilization, often reinforced, especially in the larger cities, as indicated above, by the presence of European residents.

In conclusion, I wish to repeat and reemphasize that, compared with almost any other area of the globe, the Near East is extremely poor and neglected in anthropological studies. Folklore has received somewhat more attention; of Morocco alone, we possess quite a number of high-caliber studies, dealing in great detail with the ritual, belief, and religion of the inhabitants of this classic country of magic.

Thus, the tasks ahead are great, numerous, important, and urgent. What is needed is first of all a systematic ethnographic survey of the Near East to bring our factual knowledge of it at least up to the level of our information concerning "darkest Africa." Then, particular attention should be paid to the problems special to the Near East, such as the cultural processes taking place there due to contact between various indigenous peoples, or sections of the population, and between these and exponents of western civilization Here is a wide, practically virgin field, easily accessible and promising valuable results.

Note. The Section of Physics and Chemistry held a meeting on March 2, 1948, at which Doctor John R. Dunning, Columbia University, New York, N. Y., presented a paper entitled, *Future Nuclear Research*, illustrated by lantern slides. No abstract of this has been received.

SPECIAL LECTURE

TIBETAN, TODA, AND TIYA POLYANDRY: A REPORT ON FIELD INVESTIGATIONS

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The present paper on polyandry in those parts of Asia I have visited has, so far, been given to but a few audiences, for various reasons. It was presented once in London and once in Paris, and I attempted to give it in Cairo during the war, but, interestingly enough, the Egyptian government banned the lecture because it was about polyandry—a subject which they felt might be detrimental to Moslem customs and give ideas to Moslem ladies! Happily, such a development is not likely to take place in this country.

The basic idea behind my investigations was to go out into the field and get an up-to-date knowledge of polyandry by using the psychoanalytical discoveries of the beginning of this century, attempting to work these in with our ethnological and anthropological knowledge of primitive peoples. The material gathered, therefore, will be presented here in this spirit. Unfortunately, after two years and a half in the field, war broke out and I had to interrupt my work in order to go back and fight. This, naturally, was more important at the time, but it did interrupt the work. I am planning, now, to go out again this year with a Danish Expedition, first to southern India and then to Tibet to finish off what was begun earlier.

I am sure many people are wondering why I hit upon polyandry as a special study. I chose it for the following reasons: What interests the psychologists in the West, I should say, is to get as many comparative facts as possible about the psychology of people living in other parts of the world, and especially living in an entirely different family setup, since the results we have obtained clinically in the psychoanalytical field in the West are all based upon our organization, with the father being the head of the family, the inheritance going through the father's line and the child being brought up in that environment. It is of great interest to the psychoanalysts to be able to find societies in which people live under a different family organization, since the structure of the human psyche, as found in the West, is very closely linked with the family organization we have there.

The most obviously different organization seemed to me, to begin with, to be the matriarchal family, as it is known, that is to say, one in which the mother is head of the family and inheritance goes through her family. But, by all known investigations in anthropology, it is obvious that the matrilineal type of family is one in which the mother's brother, that is, the maternal uncle, plays so important a role that the children growing up in such an environment have only a father-substitute in him. Thus, the setup which we have in the West is very similar to the one in the matriarchal family. I thought, therefore, that it would be of greater interest to go out and find something *entirely* different, even more so than the matrilineal family, and polyandry seemed to be the most radically different type or organization because, here, you have a number of fathers, indiscriminate as fathers as we know them, and one mother. The children brought up in this society are surely living in an entirely different setup from ours, and entirely different, too, from the matrilineal and matriarchal tribal societies.

Also, polyandry is little known from an anthropological point of view, and it was worth while to investigate it purely from that angle. The difficulty which I came up against in the beginning was, where to find polyandrous peoples. They are not well known and have not been extensively studied or recorded. In a book by Professor Westermark, *"The History of Human Marriage,"* which is possibly the best study of this question throughout the world, covering cases of differing family organization, it is only mentioned that polyandry exists in America to some extent, mostly in South America on the coast of Ecuador and also in British and Dutch Guiana; that it exists on the North American continent among some of the American Indian aborigines and among the Eskimos; and that it also occurs in Tibet, in India, mostly in the Himalayas, but also in southwest India, in the Melanesian archipelago, and among some primitive peoples of the Siberian plains. In Africa, it is also known in parts of Uganda and in some tribes of Southeast Africa among the Hottentots.

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Thus, I had quite a large choice of locations to study polyandrous peoples. I chose India and Tibet because, from the abovementioned book, I realized that the Tibetians had perhaps the best-preserved and most efficiently organized polyandry of them all, and that in India there seemed to be a number of unrecorded polyandrous peoples which were worth a trip to study on the spot. Then, too, both Tibet and India seemed to be healthful places for an anthropologist to work. In 1937, my wife and I decided to go out there and look for the polyandrous peoples. We visited the Tibetans for eight months in 1938, the Todas in southern India for six months in the summer of 1939, and the Tiyas on the southwest coast of India, in Malabar, for the remaining time, which was roughly about a month, until World War II broke out and we had to interrupt our activities. I shall present my findings in the same order, and end with a synopsis of the whole, giving you my conclusions as to, first, the psychoanalytical results of the investigation and, second, the anthropological results as well.

Tibetan Polyandry. We visited the western part of Tibet because, having reached northwestern India, or, rather, the foothills of the Himalayas on the Indian side, we heard that a certain amount of polyandry existed among the hill people. We went up to where they were reported to exist and found that polyandry had died out, but kept on looking further until we actually ended up on the Tibetan plateau, about 12,000 feet up, and found a very active polyandrous society among both the nomads of Rupchu (a plateau bordering on Kashmir and Tibet) and the inhabitants of Ladak. The latter is a province of what today is the state of Kashmir, entirely inhabited by Tibetans and situated in the upper Indus valley at the point where the Indus leaves Tibet and enters Indian territory. Later, we went towards central Tibet and investigated polyandry in Sikhim, which is a little state bordering on Tibet and sandwiched between the states of Nepal and Bhutan in the east.

The people living in western Tibet live in an isolated position. The country is very barren and, because of the altitude and the difficulties of travel, the people are very inaccessible. These, naturally, are excellent conditions for such a peculiar custom as polyandry to survive and I am sure that, if Tibetan polyandry is so active and well-preserved as it is there, it is due to the physical environment which favors isolation among these people. In the area we visited, 90 per cent of the population was polyandrous and I understand that nearly all the inhabitants of the vast Tibetan country, of which there are something like three million, are polyandrous, though, of course, it would take actual first-hand investigation to be absolutely sure that this is the case.

The polyandry practised among the Tibetans is of both the fraternal and the nonfraternal type, that is, when a young lady marries a young man she either marries all his brothers regardless of age (she can even be the wife of children born after her marriage), or she can take as many husbands as she desires, without their being related as brothers, at her discretion. I shall try to show why in some cases polyandry is fraternal, and in others non-fraternal.

The province of Ladak, which I visited, is inhabited by Tibetans speaking an ancient form of the language. Tibet, of course, is run by the Lamaist church and the head of the country is the Dalai Lama. The former kings of Tibet have taken refuge in Ladak since the 8th century A. D. and live in the castle of Leh. The town is very characteristic of Tibetan towns. Flat-roofed houses are very well built on the side of high mountains. The altitude is 12,000 feet, the entire area is snowed in during the winter, and the people live mostly in their houses and do not go out.

These Tibetan houses are not very different from our own western type of houses, except in one respect. The kitchen here has no chimney, so it can be imagined how much smoke fills the room when they cook. The Tibetans do not seem to mind; though we personally found it very difficult to stand upright in their kitchens when they were cooking and so spent most of our time there on all fours, to avoid the smoke and keep our eyes from watering. The people suffer from eye trouble because of this practice and yet they seem to stick to it in a very curious manner.

The characteristic dress of the Ladak women is of red cloth such as the men wear. They have a sheepskin toga which they throw over their shoulders and a very characteristic head-dress, made of a piece of red felt with two enormous ears of black wool. The red felt band is studded with turquoises at the back, family heirlooms, handed down from mother to daughter. Such a headdress is known as a *perak*.

The men are tough-looking. They very often have their right shoulders bare; though sometimes they have a Chinese shirt thrown over them. When they are working, they usually have the right shoulder uncovered, however cold the weather may be. Looking at Tibetan children, one could imagine at first sight that they have two heads, but the explanation is that they generally carry a younger brother or sister on their back because the mother is at work in the house and cannot look after them.

It is a fact that all Tibetans are literate, men and women alike. Whatever class they belong to, between the ages of 8 and 12 they have to go to the monastery to learn the Buddhist religion, learning to read and write at the same time. This gives these people a very high standard of culture.

The first thing to grasp in Tibetan polyandry is that the Tibetans are very keen to keep the property together. The country is poor, and they feel that if the property is divided among several heirs there will not be enough to go around. They not only wish to keep the property intact but also to maintain the family name to this property. Thus, when a certain number of brothers, let us say, three, inherit the property and the house, they do not divide it among themselves but, marrying one woman as they do, they share not only the property but also the woman as a common wife, as well as the children, which belong to all of them together. It is a kind of family communism, and certainly prevents division of the property.

Thus, the usual type of marriage is that a number of brothers marry one woman and keep the property. This is called a *bagma* marriage in Tibetan. However, if there are only girls in the family, what usually happens is that the younger girls go off and marry into other families, individually becoming the common wives of a number of brothers. They thus perform the regular *bagma* marriage. The eldest daughter, however, since she has no brothers, inherits both the name and property from her fathers and and is allowed to marry as many husbands as she wishes. These husbands and the children, too, take her name, so that, in this way, the children which issue from the marriage take on the mother's name and inherit the property undivided, and again make regular *bagma* marriages. The woman's marriage, in this case, is known as a *magpa* marriage. It differs from the *bagma* marriage in that the woman is head of the family. The husbands are married to her and she can do exactly as she wants with them.

The most important point to grasp in polyandry is that the house is the axis around which the system functions. In taking down genealogical notes I have, therefore, always given the family the name of the house.

A specialized case I came across was that of an only son of the house called Tsungarpa who married one woman, the heiress to the Bata house (see FIGURE 1). All her brothers were dead, so that she and her husband actually owned two houses and they had what, on the surface, looked like an ordinary monogamous marriage. However, the two properties were not amalgamated. The woman was to carry on the name of Bata and the man that of the Tsungarpa house. Consequently, of the three boys which issued from this marriage, the first two inherited the Tsungarpa house, while the youngest inherited his mother's house and was entrusted with carrying on the name of that family.

One of the sons of these three brothers, Murup, inherited the Bata house from his uncle, but remained childless after twenty years of marriage. In such a case, oddly

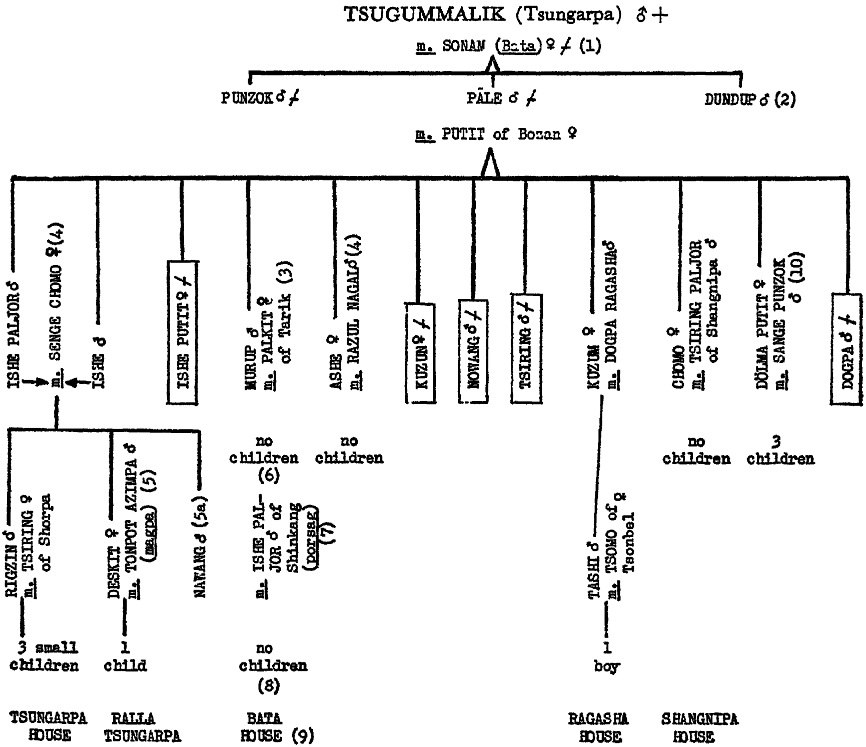

FIGURE 1. The Tsungarpa house. (1) Heiress to the Bata house—all brothers dead. (2) Put in charge of the lone Bata house. (3) Given the Bata house. (4) A Mohammedan—the woman takes the man's religion. (5) Land only held in tenancy. (5a) Becomes a Lama at Himis monastery. (6) After 20 years. (7) Murup is single-handed. (8) After 4 years. (9) Could adopt a child to carry on. (10) No land, as formerly Mohammedan; messenger at Himis monastery. † Deceased.

enough, the Tibetans seem to think that it is the husband's fault and they usually take in another man, in the hope that there will be children. Such a man is known as a *porsag*, that is, a man brought in for the purpose of fathering a child. However, when I asked this woman why she had remarried, if it was because she had no children, or because her husband was too old, she said, "No, those are only the official reasons." After I gained her confidence, she told me that she had married the second husband because she needed a servant in the house, and she found it cheaper to have another husband than to have a servant she would have to pay!

If a Tibetan woman marries a Mohammedan, she has to leave the polyandrous practice and becomes a member of the usual Mohammedan family system. The woman always takes the husband's religion, so that a Moslem girl who marries into a Buddhist family becomes polyandrous.

Now it must not be imagined that polygyny (where a man has a number of wives) does not exist in Tibet. One occasionally finds a case as when a young man goes on a long voyage and meets a girl of his choice and his brothers agree to bring her into the house. Thus, in Rupchu, I have seen three brothers sharing two wives, so that anything seems to be possible among the Tibetans and, on the whole, the system seems to work very well.

The children in Tibet are betrothed when they are 10 or 12 years old, usually by the parents. But this is not always the case. Very often the eldest boy chooses a wife himself, and his brothers and the other members of his family have to accept this woman as the common wife; they have no choice in the matter. But there is yet another custom in the Tibetan family which to us Westerners appears extraordinary indeed. As soon as the children are married, the parents are obliged to leave the house and the children take over. The father and mother must go to live in a smaller house, usually on the same property, which is built especially for that purpose. This custom is known as *kangoo, kangchen choches* which in Tibetan means "small house, big house division." The children then become responsible for the agricultural and commercial activity, while the parents must live on an allowance or on a small part of the property which is put aside for them (called *maskal* if it is for the mother, and *paskal* if it is for the fathers). This custom, though not rigidly enforced, seems very strange to us, since in our society the parents remain in charge of the family property until they die and the children have to go and earn a living for the family which they found.

It cannot be said that, in a Tibetan polyandrous family, one of the brothers is *eo ipso* more important that the others. For instance, it is not necessarily the eldest brother who is the head of the family, but usually the one with the most personality and sometimes even the mother, if she is in a position to run the family. The fathers are often somewhat afraid of their sons, because as soon as the sons become married, the parents have to leave the house. Thus, they try to ingratiate their children so that they will be left a sufficient amount of property on which to live when the children marry.

There is an initiation ceremony among the Tibetans, as with many other peoples in the world. It consists in the boys and girls having their hair cut at an early age, usually between their second and third year, by the mother together with one of the fathers, usually the most influential one.

There seems to be no fixed rule as to how a polyandrous family works out relations between husbands and wife. Most people seem to imagine that it is left to the wife to regulate with whom she shall have relations, but this is not always the case. If it is left to her, she naturally has to use tact so that the boys are all satisfied, but, in other cases, the decision is up to the brothers themselves, who remain at home, in turn, while the others go off on commercial journeys. This idea of one brother staying in the house and looking after the common wife is what many anthropologists believe to be the explanation as to why these people practice polyandry; that is, so that they can be sure the children issuing from the marriage will be at least from the same family, even if they are not those of one particular man. Thus, during the war, I saw a Sikh officer (an Englishman) who received a request from one of his soldiers to go home because, he said, his wife had had a child. The officer said, "But you have been away for two years, so how can your wife have had a child? Is that any reason to go back?" And the Sikh answered, "Yes, but my brother was at home," and then retracted his words, because the Sikhs, at least, do not like admitting they are polyandrous, though it certainly exists in the northern part of the Punjab, among them as well as among the Hindu *jats*.

In Leh, I was struck by the fact that, in most families, there seems to be no organization at all. Complete promiscuity apparently exists between the brothers and the common wife, especially during the winter months when the place is very often snowed in completely and the whole family lives in one room because they have not enough fuel to heat the whole house. This I found out both from questioning the people and from hearing details from Europeans who have gone into the question before.

Oddly enough, however, in all anthropological books about polyandry, one hears that the brothers, in turn, go in to the wife and signal their presence in the nuptial room to the other brothers by leaving some mark at the door. They either put their sword against the door or their stick, or leave their shoes or hat outside, or hang their beads on a hook at the door as a signal that they are in with the common wife and the other brothers may have no access to the room. When I examined this first-hand, however, and also in the case of other polyandrous peoples like the Todas and Tiyas, I found that they say this is not practised any longer, but that it used to be a custom two or three generations back. I have never found a case of its actually being practised today and, for this reason, I have strong psychoanalytical suspicions that it has never been practised, but that this story has been put out by polyandrous people as an indication of, what I should call popularly, "wishful thinking." Each of the brothers would like, in some way, to have the common wife all to himself and would like to manifest this virility on his part by hanging up some emblem at the door. The sword, the stick, the shoes, the hat, and even the beads are all, psychoanalytically, symbols of virility, and it seems as if the men would *like* to have this custom which, possibly, has never existed, and each of them gains a certain satisfaction in thus imagining a virility which he feels he does not have.

The morality of the polyandrous people is very high. I mean by this that, as polyandry is an institutionalized social system, they stick to it very strictly. If you ask missionaries who have been among polyandrous people, they will, of course, tell you that polyandry is a very immoral practice. But if you speak to the people themselves, they, on the contrary, consider it highly immoral not to be polyandrous. If we take as the definition of morality, conformity to the social standards of the setting in which one lives, then one can say that polyandrous people, at least in this area which I studied, are very moral. The old people complain that today the young ones are getting highly immoral and lax in not carrying out the tradition of polyandry. Certainly, on the borders of India, where the people come in contact with the monogamous families of India, or with Europeans, polyandry is dying out to some extent.

I remember speaking to an old Tibetan called Tocktock in Leh, who told me that, in town, Indian and Turki traders (the latter from Chinese Turkestan), and European travelers were introducing the bad habit of monogamous marriage, and that the young men were getting more and more immoral. I suppose he was quite right, and as I was interested to hear his point of view we had quite a conversation about it. He next asked me how, in Europe, we managed to remain wealthy without being polyandrous. He said to me, "Here in Tibet we haven't enough to go around and as we don't want to divide the property up we have to be polyandrous so that we inherit the property without its being divided. But how do you Europeans do it? How do you remain wealthy by being monogamous and dividing up your property?"

I must say I had not thought of this before. It only occurred to me when I was talking to this man. So I said, "Yes, now that I think of it, it is probably true that in Europe we do things in a different way. When we marry, each man goes off with his wife and they try to fend for themselves, build up their own family and get their own source of revenue. That is the way we do it. There is enough wealth to go around to found other families." "Yes," the old man said, "That is true. I have noticed among the Tibetans who have become Moslems that this is also the case. They cease being polyandrous, and because of that they then divide up the property. There is not enough to go around. The eldest brother usually gets it and the youngest brothers have to go off with their wives elsewhere to find some work. I have also noticed here that nearly all the Mohammedan families seem to go to Burma and work in the ruby mines there. That is obviously for the same reason you are telling me now about Europeans." Then he was silent for a moment and looked at me in a pensive fashion until suddenly he said, "Now I understand why you Europeans come here. That is why you have got colonies. If you were polyandrous you wouldn't need to!"

One question I am very often asked is, does jealousy exist within the various houses? I have the impression that it does exist but that it is very highly repressed by this institution of polyandry. Tibetans, when asked if they are jealous among themselves, will always answer that they cannot afford to be jealous because of the economic sanction against it. If they started quarreling among brothers, they would have to divide up the property. They would then be too poor to carry on, and this threat makes it impossible for them to show their jealousy.

As it is, there seem to be some outlets for it. I have noticed a number of what one might term "unjustified" *magpa* marriages, cases of young men who were married jointly with their brothers to one woman and yet each went off and married an heiress. Such a man could have remained married to the common wife, but was lucky enough to find an heiress he could marry and he certainly did so at once, because it gave him some justifiable motive for having a wife all to himself. There are very many such cases of *magpa* marriages which are not justifiable, in the case of the males, except for the desire to break away from a polyandrous family.

A certain amount of neurotic cases among brothers can also be noticed, which shows that jealousy certainly exists unconsciously in their psyche. I saw a young man in Keylong, in Lahaul, where we were staying for a month (or, rather, I heard about it because it had just happened when we arrived there), who had committed suicide because he felt that the common wife was not giving him enough chance of access to her. He was in love with her and his other brothers were obstacles, so he committed suicide. In Leh, I actually saw a young man who for many years, as long as his brothers were alive, had suffered from a paralysis of one of his legs. His brothers died off one after another until he was left alone with the common wife, and queerly enough, that very day he lost the lameness in his leg, so that it looked as if it had been a hysterical manifestation due to repressed aggression within him, certainly caused by jealousy of his other brothers for their relations with the common wife.

There are certain instances of extramarital relations between people in this polyandrous community, but they are highly condemned and heavy sanctions are applied. In a *bagma* marriage, if the woman commits adultery, she is turned out of the house if she is childless, and the brothers choose another wife. If she is with child, she is fined and has to pay out of her personal property. As for the man, the co-respondent, he is beaten and/or has to pay compensation to the legal husbands. In a *magpa* marriage, where the woman is heir to the property, has the care of it and can marry as many men as she desires, infidelity of the wife is the only case in which the husbands can get a divorce. Otherwise, she can divorce them, but not *vice versa.* If the man is the adulterer in such a marriage, nothing is done except in the family where he has committed the adultery. In the case of unmarried people, the woman is usually turned out of the house if she has premarital relations and should attempt to marry the man with whom she has had these relations. If the trespasser is a man, he is treated exactly like an adulterer in the usual *bagma* marriage.

Illegitimate children are usually hushed up. They are often adopted into some family or are sent to the monasteries as novices, so that nobody knows exactly what their origins are. As far as I could find out, there seems to be no prostitution in Tibet, and husbands suffering from the polyandrous organization do not have that outlet. Something, however, certainly is known about prostitution, for Tibetans, who never smoke, tell you that they do not do so because tobacco was a plant grown with the menstrual flow of a prostitute.

As far as I could find out, homosexuality is condemned, even in the monasteries, and certainly despised if practised, and I could not find any proof of it. But local Europeans who know the Tibetans very well, such as the Moravian missionaries who live among them in the western part of Tibet, explain that, on the whole, they are not very passionate people, but rather cold. Certainly, their polyandrous institution has much to do with the repression of their natural feelings.

Oddly enough, the Buddhist religion condemns not only polygyny and divorce, but also polyandry, and yet all Tibetans who are Buddhists practise it. Thus, it must be a pre-Buddhist custom. The Moslems, however (that is, those Tibetans who are converted to Mohammedanism) cease being polyandrous immediately and take to the Islamic polygynous system.

There exists a very rare case of polyandry in Tibet which it has not yet been my privilege to come across, but of which I heard repeatedly in western Tibet. It is practised in a society which I understand exists in central Tibet around Lhasa and Gyantse. In these towns, the fathers and sons share one wife. One of the objects of my returning now, at the end of the year, to Tibet, is to meet a woman who is thus married to both father and sons. Such a woman is known in Tibetan as a *tchamadoong,* which means "beam-mother-beam" a name derived from an analogy in the build of their houses. These have flat roofs, built over a number of beams, large transversal beams and others, which are smaller, going across them, and very small bits of wood which link the two together. When they call such a woman a *tchamadoong,* they mean she is comparable to a small beam, that she links the big beam with the small bits.

It must not be imagined for a minute, of course, that this woman is the mother of the sons. But let us assume the case of a family in which there are a number of brothers, who have married one woman and have a number of offspring. If this mother dies, the fathers can either remarry and, in this case, the sons are entitled to share the stepmother with their fathers, or, if the sons are married when the mother dies, the fathers, instead of remarrying, are entitled to share the daughter-in-law with their sons. That, as I say, is practiced only in central Tibet but is well known in the entire country. The children issued from such a marriage legally have as fathers those to whom the wife was officially married at the moment of the birth. That is, if widowers share their daughter-in-law with their sons, the children of that woman are the children of the sons, so that your legal grandchildren can be your own physiological children and, *vice versa,* if it is the fathers who are legally married to the woman, your legal brother can well be your own physiological son!

I understand that Tibetans from other parts of the country rather despise this form of marriage. I have caught my caravaneers openly insulting central Tibetans as being *tchamadoong*-practicing. Also, when they are playing dice, I noticed that sometimes they invoke the *tchamadoong* to produce luck for them, so that she is a double kind of figure to whom they cannot be entirely indifferent: they despise her and yet, at the same time, she can bring them luck.

I do not know how far the existence of the *tchamadoong* has been reported in anthropological circles heretofore. Personally, I have never heard of this system before, except on the spot, and I am very keen to go back and study it. The case to which we came closest was the governor in the western province of Tibet, named Ngari Korsum. He was living in the capital, Gartok, when we were there. He was a central Tibetan sent by the central government to govern that province, and with him were his two sons and their one wife. She was a *tchamadoong,* the wife of the boys, the father sharing her with his sons. I was shown a photograph of her, taken by a Tibetan who had a camera from India, and that is the closest I have been, so far, to such a woman. I understand that in southern India there is a similar practice, in which a father is entitled to enjoy his daughter-in-law as long as his son is a minor, but this practice is not exactly the same for it is not an institutionalized form of marriage, the way the *tchamadoong* is in Tibet. A monk I met at Spitook, a monastery close to Leh in Ladak, told me that he knew five families with *tchamadoongs* in Gyantse. I have noted their names and hope to be able to study them on the spot.

Toda Polyandry. I shall now try to show how polyandry, as practiced by the Todas, differs radically from the Tibetan system. This is a worth-while consideration, for it shows how polyandry, though it is basically the same organization, can appear in different forms throughout the world.

The Todas are probably the best-known primitive people in the world. Anthropologists mention them repeatedly. They live in the southern Indian hills, called Nilghiris, west of Madras. There are very few of them left, only about 600, and they were very well described by Doctor Rivers at the beginning of this century, though much could still be learned about them, as Rivers admits himself. He made many discoveries, but also made several mistakes. In his book, the chapter on religion tells the amusing story of some investigators who had been there before, who tried to note down the names of the various gods the Todas have in their shrines. One god has a very long name, which, to this author, seemed to be correct, but, as Rivers pointed out in his book, the words that appeared as the "name" really mean, in Toda, "Give me another rupee and I will tell you what this god is called." It is easy to understand what happened. I think that similar progress could be made in the investigation of Toda polyandry, but that Rivers did not go into it completely and sufficiently.

Unfortunately, as I have said, the Todas are a dying-out people, there being only 600 left, of which only about 50 per cent are polyandrous, *i.e.,* a much smaller proportion than among the Tibetans. The Todas are divided into two main clans, a superior leading class called the Thartars, and an inferior clan which serves the Thartars, known as Teivil. They have no landed property like the Tibetans, but are herdsmen and have large herds of buffalo, some of which are sacred. In this case, therefore, we do not find the polyandry grouped around landed property which is not supposed to be divided. The Todas only practise fraternal polyandry, and when I asked them if they practised non-fraternal polyandry, as in the case of the *magpa* marriage among the Tibetans, they were very shocked. They thought it was a very immoral idea that two men, not being brothers, could have one wife in common.

The country in which these Todas live is not barren like Tibet. On the contrary, they live in a very wooded area. Part of the present Todas actually live on a golf course, because British troops have quarters in this area and the officers have set up a sporting club which includes part of the Toda territory. The houses are very small, built only for protection, and radically different from any native construction which their neighbors have. I do not know where they got this architectural style, and nobody has been able to trace its origin. There is a rather tiny door through which one has to bend to enter. The house is all thatched, very well built, and quite snug inside. The front is made of boards, and there is a wall around for protection.

Men and women alike are dressed in long, flowing, white robes, with red and black stripes on them. They are of an obviously Indian type, rather good-looking. Some of them have progressive ideas to the extent of wearing spectacles, because they are either far- or short-sighted. They are quite good physical specimens of humanity, the color of their skin being dark brown and their hair jet black. The men are very hairy. Some of them wear turbans, but usually they go about bareheaded, showing a rich growth of black hair which they all have, young and old. I wondered if they had a secret formula for hair-dressing but they do not seem to have any except buffalo butter, which they rub into it. They are very fond of sporting games, and the country is green and rolling with many hills and is very nice to play about in.

A Toda genealogy, analyzed in detail, will best enable us to compare their system to the Tibetan. As I said at the start, the property here is not the axis around which polyandry turns.

Three brothers married, polyandrously, a young lady, Potöz (see FIGURE 2). She was married to one of her cousins when she was two years old, as is the practice. When she came of age, however, she married these three brothers. She had no children and died quite early, so that the three brothers were left without any children and without a wife. Then, the following thing occurred. The eldest brother Eroï, married a young woman, known as Sotaboof, and went off on his own; *i.e.*, he was not willing that his two brothers should share this woman with him. He became a government agent, and because he had founded a family of his own, he was entitled to take his part of the buffalo belonging in common to all three brothers. He took one-third of the buffalo away, and two extra, although I cannot remember why; but the important thing is that he was entitled to a share of the common property.

The two others tried to marry a woman called Mab. The second brother, Kiel, asked Mab's father, but the father started haggling with him over the bride-price. (They have to pay a price for a girl, a custom which does not exist among the Tibetans.) The girl's father wanted nine buffaloes, which the young man would not give and left in a huff. Then the third brother, Konoch, married Mab, and soon after she became pregnant. In the meantime, her father died and the second brother, hearing of this from Konoch, returned to the house and asked if she would accept him as her second husband, which she did, even though she was pregnant from the third brother. The

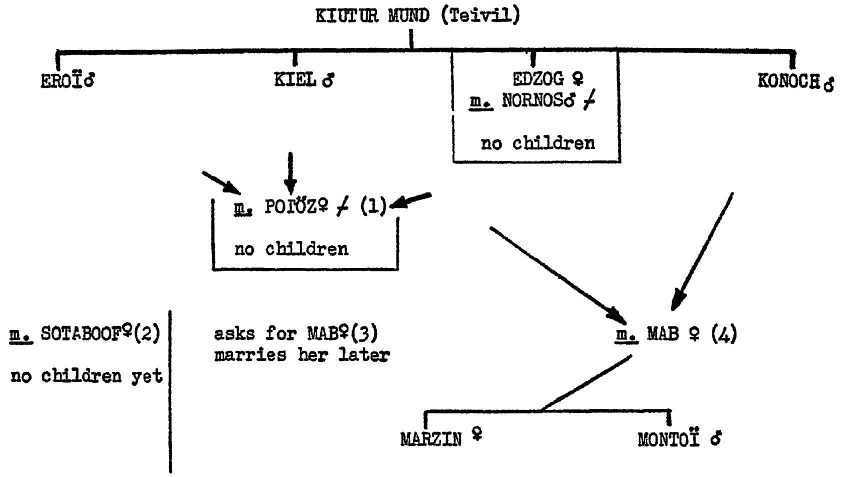

FIGURE 2 A Toda genealogy. (1) First married to Kazan when two years old (2) Becomes a government peon, takes one third of the buffalo plus two away (3) Quarrels with her father over bride price, leaves with one-third of the buffalo but comes back and marries Mab after his brother, Konoch, has already married her (4) Konoch originally is left with one-third of the buffalo (four) Mab becomes pregnant before Kiel joins in the marriage † Deceased Arrows denote polyandrous marriages

second brother had also left, when he ran away in a huff, with one-third plus two of the buffalo, which he now merged again with the third brother's, so that two-thirds of the original buffalo (minus two which went with the first brother) again became the common property of these two brothers. They thus had one wife and later shared the two children born of the marriage, a girl, Marzin, born shortly after the polyandrous marriage, and a boy, Montoï, of whom the mother told me that she did not know which of the two was his father, and that she had no preference, because she was a good polyandrous wife to both.

The Todas do not practice polygyny as the Tibetans do. They were shocked that I should even mention it. Brothers who have left the house (like the first one in the above example) are not allowed to return with some other wife and share her with their brothers. This is never practised and, even when a brother leaves, he is only given one chance to come back alone to the family fold. Should he do it a second time, he is turned out of the house and the brother or brothers living with the wife are exclusively entitled to her.

Instead of the Tibetan practice, in which the children are betrothed by the parents early, the Todas have a custom of marrying first cousins very early, as child marriages are common nearly all over southern India. In such a marriage, the betrothal would likely take place at least in the first or second year, and always between cousins, the boys marrying paternal girl cousins and the girls marrying maternal boy cousins. Later, these marriages are dissolved by divorce and the children are entitled to remarry by inclination, but compensation is always paid to the "legal" husband. This is worked out in a traditional manner, so that it is certainly a survival of some older form of marriage.

Another difference with Tibetan polyandry, in the case of the Todas, is that one man is usually declared the father of the children, though this is not always so. In the aforementioned case of Mab, the woman said she had no preference as to who was the father of her children. The woman is regarded as part of the goods and chattel of the men. She is looked down upon, considered as part of the property, as part of the household, and must pay obeisance to her husbands. She does this by bending down and putting her head under the feet of the husbands, which is the typical Toda salute of respect. There is an initiation ceremony very similar to the Tibetan one, in which the boys' and girls' hair is cut early and even one of the ears is pierced. However, here it differs from the Tibetan custom, in that it is the maternal uncle who does this, which is rather curious and may show some influence of a matrilineal, matriarchal nature.

Particularly extraordinary among the Todas is the fact that no girl of the Thartar clan (the superior section of the Todas) is allowed to be married as a virgin. She has to be deflorated first by a member of the Teivil clan. This seemed to me to be so extraordinary that I decided to look into it a bit more closely. Teivil girls can marry as virgins, but the Thartar girls must be deflorated first by a Teivil before they can marry a Thartar. Naturally, that kind of thing is difficult to find out about, because the people are too shy to speak about it. When I finally got hold of an old Toda and asked him what the reason was, it took me at least a month to find out. I gave him a lot of presents, among other things a bag of rice and a calf buffalo, and still he would not speak. Finally, I found out that it was not that he was unwilling to do it despite the presents I had given him; on the contrary, he would have very much liked to tell me about it, but seemed to be scared to speak about it in the Toda area. He was afraid of the spirits and the Toda gods if he revealed so intimate a secret. Consequently, I decided to take him miles away, out of the Toda area, where his gods and spirits could not get hold of him. We drove at least 100 miles away from the Nilghiris before we stopped by the side of the road and I again asked him, "Why do you practice this custom?" His answer was very interesting. He said that, if they did not do this, the maternal uncle of the girl would die. This seemed very surprising, so I asked why. He said, "I do not know. It is like that. If she were allowed to marry as a virgin, her maternal uncle would die." When asked what would happen if the girl actually did marry as a virgin, he said, "Sometimes it happens, and there is just one chance again for the maternal uncle to save himself. That is, he should take the young girl and shave all her hair off. It is the only chance he has of saving his life."

I thought, from a psychoanalytical point of view, this was most interesting because it seems to me as if there were a feeling of guilt in the defloration of such a young lady; that the Oedipus complex comes out here although it has been transferred to the maternal uncle. He is the one who is threatened by consummation of the act (as a father would be in a son-father sexual conflict), and he can only save himself by castrating the girl (displacement of the suitor?) by cutting off her hair. Those are hypothetical psychoanalytical interpretations, I know, but they seem to me strong indications that these people have probably much the same complexes that we have in the West.

Relationships between the husbands and the wife among the Todas are different from those among the Tibetans. They are not promiscuous in any way, and in their marital relations the brothers go strictly in turn. They, too, will tell you that, in past times, their grandfathers (or ancestors still further removed) used to leave their stick at the door, or their shoes, or something, always to show they were there. This is not practiced "any more"; the brothers know by intuition that one of them is with the common wife, or he tells the others openly that he is going to do it.

As far as morality is concerned, polyandry is not enforced in the same way as among the Tibetans. It is somewhat lax and usually applied only to the woman. The men perpetually seem to be stealing one another's wives, and it happens that they can even retain such a woman they have stolen from other families, if they pay compensation to the husbands. This goes on continually and one sees some women who have been to three or four families in turn. The Todas do not seem to be any more jealous than the Tibetans, but perhaps this very laxity gives an outlet for jealousy, since they can steal one another's women, so that this does not appear to be a good instance for the study of jealousy among a polyandrous people.

There are numerous extra-marital relations, which are tolerated as long as they take place among Todas. At the funeral of a Toda woman, it is curious to note that there is one individual who officiates in addition to the husbands, and usually he is supposed to be the official lover of the woman. This looks peculiar, since they have it only in a ritual, and it may indicate that they once practised non-fraternal polyandry and that this official lover who officiates together with the husbands is perhaps only a survival of a non-fraternal mate.

Pre-nuptial relations are forbidden among both men and women, and there are never any illegitimate children among the Todas, as they are always born within one family (since "marriage" takes place in infancy) and considered legitimate. The men alone are allowed to divorce, but women may never do so. The usual ground for divorce is that they have stolen one another's wives. The only case in which a woman is divorced with ignominy is if she has had relations with a non-Toda. In that case, she is driven from her home, her hair cut, and usually tied to a buffalo with coconuts attached to its neck, after being beaten by the community.

The impression I got was that Toda polyandry was loose and decadent, and that, today, we see only the remnants of a former, more efficient type of polyandrous organization.

Tiya Polyandry. The Tiyas, living in Malabar, exhibit one especially interesting phenomenon: they are a polyandrous people who are patrilocal (that is, the woman marries and goes to live with her husbands), but also matrilineal (that is, the property that they have—and they do not have much, because they are common laborers—is inherited from the mother's brothers). Unfortunately, I did not have much time to investigate these people because the war broke out when I had only been at work among them for three weeks. Thus, the only thing I could note was that they are polyandrous, patrilocal, and matrilineal at the same time. I should like to return and see how this works out.

General Conclusions. I am afraid that some of these results may seem superficial. I was unable to stay as long as I should have liked among all these peoples. I did not know any of their languages well, and to really do good field work, one would have to live among them and not put questions to them as I did. However, I hope to return and to do a more thorough study.

As far as the psychoanalytical results are concerned, I think that they are highly illuminating, though not sufficiently ample to be conclusive. There are strong indications that, broadly speaking, psychic mechanisms, libidinal development and mental structure may be identical to those in the West. There are, thus, numerous instances of trends pointing to Oedipus and castration complexes. What I, however, really investigated insufficiently was where we should place the relative borderline between what is specifically culturally determined in psychology (such as traits due to family environment) and what is generally humanly so (such as sociobiological traits).

As far as the ethnological results are concerned, these can be examined from two different angles. Functionally, polyandry fulfils the same purpose as all other marriages: it is a social institution for the satisfaction of the individual sexual instinct and for the orderly social procreation of offspring. Formally, it is more difficult to say why this institution has taken on this curious appearance. As far as I know, authors so far have been rather at a loss to furnish a satisfactory explanation. Many, of course, have been given, with a great amount of variety in their choice.

For instance, we are given historical explanations. In India they say people have been polyandrous for many centuries; that there are indications, such as archaeological (about the Heftalite Huns, 6th c. A.D.) and literary (the Indian epic poem, the Mahabarata), that the people in this area, both Aryan and non-Aryan, were polyandrous in antiquity. Economic reasons for this have been given; *i.e.*, that the country is poor, that nomadism among Todas or Tibetans, in earlier times, perhaps led to a polyandrous society in order to limit the number of children. Then, demographical explanations have been given, that there is an excess of males over females in this society, and that they have to be polyandrous for that reason. For instance, the Todas practise female infanticide and this is given as a reason for their polyandry. Their men drink a lot of milk, and some people say that the fact that they have more proteins in their diet in this fashion produces more male offspring than female. I do not pretend to know how far this biological explanation is true. Sociological reasons have also been given, namely, that the men travel and have to leave one brother at home with the wife (a reason which I hinted at earlier). In east and south India, the people known as the Nayars, who once could also be called polyandrous, had a very definite military organization (somewhat like that of the Sikhs in the north), and that has been given as a reason why a people is polyandrous: one brother can always remain at home with the wife while the other is away serving with the colors.

It seems to me that these explanations are not sufficient. As far as the historical reasons are concerned, the Mahabarata epic is the only case of its kind in the whole of India. The Todas, when you ask why they are polyandrous, always say "Because we are the same people as those of the Mahabarata", but I am convinced that they have got this explanation from the anthropologists and not the other way around! As far as the economic reasons are concerned, these also seem to me insufficient, because there are people living in the same area, in the same physical environment, who are not polyandrous. Even the Tibetans, when they become Moslems, though their environment does not change, suddenly cease being polyandrous and live in the Islamic fashion. The rich Tibetans in central Tibet are also polyandrous, and obviously not for economic reasons.

As regards the demographic reasons, the following statistics will show that there is not always an excess of males over females. In the Leh *tehsil* in Ladak, where I was, there are 14,760 men to 14,650 women, which is approximately equal. The excess of single women among the Tibetans (there are some left over, naturally, considering the number of brothers who have one wife) usually go into a monastery and become nuns. That is the way they solve their problem. The Todas, on the other hand, practise female infanticide, so that there are less women than men. However, back in Tibet, in the same town of Leh, one finds that the Hindus who live there number 36 men and 13 women, and yet they are monogamous. In southern India, there is a Moslem community known as the Moplahs, in which there are five men to one woman, and they are polygynous rather than polyandrous. Even in Europe, the Bulgars have 35,000 more men than women and yet, though they may have many faults, they certainly are not polyandrous.

As far as the Todas are concerned, out of the 600-odd that exist, there are 340 men and 237 women, so that it really looks as if the reason given might be true in this case. But as Rivers has very well pointed out—"which comes first?" It is the old problem of the egg and the hen. Do they kill off their little girls because they wish to remain polyandrous, or are they polyandrous because they have more men than women owing to the practice of female infanticide? If the latter, the fact that they can steal one another's women would be another solution to the problem, so that again the demographic reasons seem to be insufficient.

Being unsatisfied with these explanations of polyandry given by various authors, I decided to find out from the people themselves why they are polyandrous. I do not think this has been done before. Most people find out about polyandry and then they try to think up some explanation for it, but no one has gone and asked the people themselves. Of course, when I checked the scientific explanations against their ideas, I got various answers.

There was no other answer from the Todas about the Mahabarata than that which I have already discussed. As far as the economic reasons are concerned, the Tibetans did say yes, they lived in poor surroundings and that is why they are polyandrous. Demographic reasons (that there are more men than women) were never mentioned, while sociological reasons such as frequent traveling (one brother having to remain at home with the wife) were mentioned often. But I was struck by the fact that, of the 100 answers I got, at least 95 were quite different from all the scientific explanations.

The usual answer I got was, "We are polyandrous so that our wives will not quarrel." The Todas even said that they are polyandrous because they do not want the *brothers* to quarrel with one another. And I must say that I found this explanation by the people themselves much more convincing, from a psychological point of view, than the academic explanations of certain authors. Basing my own view on the answers of the people themselves, I would give the following interpretation of the polyandrous systems found: It seems to me that polyandry is "not necessarily a primitive institutional form of marriage, but that it is due to the psychological reaction of individuals living in communities subject to environmentally poor and insecure economic conditions, persisting, in the case of the Tibetans, through isolation from other communities, or, in the case of the Todas, as a type of defense mechanism against invasion by other cultures." I have not included the Tiyas in this definition because I do not know them sufficiently and would like to go more thoroughly into their polyandry before giving an interpretation of it.

Now, if I take this definition, instance by instance, I shall be able to interpret exactly what I mean by it. When I first say that polyandry is "not necessarily a primitive institutional form of marriage," I mean that, for instance, the Tibetans are all literate and educated people, with whom you can speak quite freely, who have a very pronounced sense of reality and do not in any way seem like a primitive people. It thus looks as if polyandry were a system entirely outside of the usual evolutionary stages of marriage, in which anthropology puts the matriarchal before the patriarchal system.

Next, I say that polyandry is "due to the psychological reaction of individuals living in communities subject to environmentally poor and economically insecure conditions," by which I mean, living in a physical environment which leads to a miserable economy. This economy makes them very interdependent, within each family, upon the help of each other, and this interdependence, in turn, which is stressed very highly in these families because of the necessity of sticking together under the difficult physical conditions, calls for less aggression among the members of the family itself and among groups of them. It seems to me that this stress on the condemnation of aggression is such that even something approaching incest appears as less dangerous than aggression among brothers. In fact, the balance between aggression and love for one another is upset in favor of love, and this is so effective that latent, repressed incestuous strivings are forced out by an excuse, I should say, of loving one another and are less condemned than aggression between brothers.

I am convinced that it is this feeling that finds its expression in the Tibetans and Todas saying, "We are polyandrous because we do not want to quarrel among ourselves." (The Tibetans say that they do not want the women to quarrel, by which indirectly they mean that, if the women are quarreling, the men will have to take sides and would quarrel. The Todas are more definite about it and say outright that they are polyandrous because they do not want the brothers to quarrel.) These people actually feel that a practice of aggression, of rivalry between brothers is more dangerous than the custom of sharing one woman, which has led to the establishment of a community in which it has become compulsory to live in this fashion.

It is very characteristic of polyandrous people that they insist on being polyandrous and say it is immoral to be anything else. In fact, this latent eruption of a repressed incestuous desire becomes an obligation, which is very much in line with the teachings of psychoanalysis; namely, that a repressed desire becomes the basis of a compulsion, in this case of an imposed institution, which it would be difficult to explain sociologically. Of course, these incestuous obligations, as you might call them, do not go as far as a father-son relation, but it seems to me that the *tchamadoong* system is so close to such a relation that again it could easily be explained in this way.

In the final instance, as I say, polyandry "persists through isolation, in the case of the Tibetans, and as part of a defense mechanism in the case of the Todas." The Tibetans are very isolated and do not come in contact with other peoples and, for that reason, the system is well preserved, though in the border region it is breaking down. I was much struck by the fact that people there do not like mentioning polyandry. They are shy about it and think it does not sound very nice to other people, while as one gets into the interior among real polyandrous people they are very proud of it and boast of it. In the case of Tocktock, which I mentioned before, the old man was surprised that other peoples were not polyandrous. As far as the Todas are concerned, they are cooped up in the mountains. They are hemmed in by other peoples who have other customs, but they still feel that they are the people of the Nilghiris, and that they are a superior race. In order to assert this, they stick very "nationalistically" to their customs, and I think polyandry persists among them as one of the symbols of their superiority.

I hope that this explanation of polyandry may sound convincing in spite of its mainly psychoanalytical approach. I think that the results of this first expedition were satisfactory, and thoroughly justify the desire for a second expedition to find out more about it, for there are many gaps that still need to be filled in, in order to complete the picture.

CONFERENCES HELD

SECTION OF OCEANOGRAPHY AND METEOROLOGY

CONFERENCE ON "OCEAN SURFACE WAVES": MARCH 18 AND 19, 1948

The Section of Oceanography and Meteorology held a Conference on "Ocean Surface Waves." Doctor Bernhard Haurwitz, New York University, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Thursday, March 18

Morning Session. "The Solitary Wave and Periodic Waves in Shallow Water," by Joseph B. Keller, Institute for Mathematics and Mechanics, New York University, New York, N. Y.

"The Action of Floating Bodies on Ocean Waves," by Fritz John, Institute for Mathematics and Mechanics, New York University, New York, N. Y.

"Breaking of Waves in Shallow Water," by James J. Stoker, Jr., Institute for Mathematics and Mechanics, New York University, New York, N. Y.

"Application of Solitary Wave Theory to Surf Problems," by Walter H. Munk, The Scripps Institution of Oceanography, La Jolla, California.

Afternoon Session. Chairman, G. E. R. Deacon, F.R.S., Admiralty Research Laboratory, Teddington, Middlesex, England.

"Wave Motion at the Surface of a Current which has an Exponential Distribution of Vorticity," by A. J. Abdullah, Massachusetts Institute of Technology, Cambridge, Massachusetts.

"Determination of the Depth of an Underwater Explosion from Measurements of the Dome of Spray," by C. L. Pekeris, Department of Applied Mathematics, The Weizmann Institute, Rehovoth, Palestine.

"Notes on Surface Waves," by Maurice Ewing and Frank Press, Department of Geology, Columbia University, New York, N. Y.

"The Propagation of Small Surface Disturbances Through a Non-Potential Flow," by P. D. Thompson, Institute for Advanced Study, Princeton, N. J.

Friday, March 19

Morning Session. Chairman, Roger Revelle, Office of Naval Research, Navy Department, Washington, D. C.

"Recent Studies of Waves and Swell," by G. E. R. Deacon, Admiralty Research Laboratory, Teddington, Middlesex, England.

"Some Results of Analyses of Sea Surface Roughness in the Western North Atlantic," by H. R. Seiwell, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"The Comparison of Forecast and Recorded Waves on the Pacific Coast," by J. D. Isaacs and Thorndike Saville, Jr., University of California, Berkeley, California (read by M. P. O'Brien).

"Variability in Direction of Wave Travel," by R. S. Arthur, Scripps Institution of Oceanography, La Jolla, California.

Afternoon Session. Chairman, Columbus O'D. Iselin, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"Naval Application of Wave Research," by A. C. Vine and Roger Revelle, Navy Department, Washington, D. C.

"Ocean Wave Research and its Engineering Applications," by Martin A. Mason, Beach Erosion Board, Army Engineers, Washington, D. C.

"Details of Shore-Based Wave Recorder and Ocean Wave Analyzer," by Arthur A. Klebba, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"Utilization of Wave Forecasting in the Invasions of Normandy, Burma, and Japan," by Charles C. Bates, U. S. Navy Hydrographic Office, Washington, D. C.

NEW MEMBERS

Elected March 25, 1948

SUSTAINING MEMBERSHIP

Levine, Emanuel, M.Ed., Paleobotany, Paleozoology. Professor of Biology, Rider College, Trenton, N. J.

Vacquier, Victor, M.A., Geophysics, Instrumentation. Research Engineer, Sperry Gyroscope Co.; Research Associate, Geology Department, Columbia University, New York, N. Y.

Youngstein, Max E., M.L., Medical Motion Pictures. Vice President in Charge of Advertising, Publicity, and Exploitation, Eagle Lion Films, New York, N. Y.

ACTIVE MEMBERSHIP

Aloysius, M., M.A. Instructor, Biology, Chemistry, College of New Rochelle, New Rochelle, N. Y.

Badian, Albert A., Sound, Time, and Temperature Controls. Factory Manager, Comet Slide Fastener, Inc.; Treasurer, Seaboard Engineering Co., New York, N. Y.

Baum, Sidney J., M.A. Production Manager, Sterile Solutions, Endo Products, Richmond Hill, L. I., N. Y.

Benjamin, Bernard, M.D. Assistant Professor of Clinical Pediatrics, Long Island College of Medicine, Brooklyn, N. Y.

Bennett, Harry, B.S. Technical Director, Glyco Products Co., Inc., New York, N. Y.

Benson, Sidney W., Ph.D. Assistant Professor, Department of Chemistry, University of Southern California, Los Angeles, Calif.

Braham, Stanley B., M.D. Interne, Mt. Sinai Hospital, New York, N. Y.

Chase, Richard G., Ph.D. Chemist, Merck & Co., Inc., Rahway, N. J.

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Coss, James A., Jr., M.D., Arthritic and Rheumatic Diseases. Assistant, Medicine, Columbia University, New York, N. Y.

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Friedman, Leo, M.S. Biochemist, Vitamin Division, U. S. Food & Drug Administration, Washington, D. C.

Friedman, Paul, M.D., Ph.D. Associate in Neuropsychiatry, Beth Israel Hospital, New York, N. Y.

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Gurd, Fraser N., M.D. Lecturer in Surgery, McGill University, Montreal, Canada.

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Jackson, Harold R., Firearms and Bladed Weapons. New York, N. Y.

Jackson, Raymond S., M.D. Instructor, Internal Medicine, University Hospital, Ann Arbor, Mich.

Kapp, Roland, Ph.D. Manager, Fine Chemicals Division, Nopco Chemical Co., Harrison, N. J.

Krasner, Leonard, M.S. Clinical Psychology Interne, Veterans Administration; Ph.D. Candidate, Columbia University, New York, N. Y.

Leeds, William G. Medical Student, Long Island College of Medicine, Brooklyn, N. Y.

Lefkowitz, Arnold, P.E., Structural Design and Metallurgy. Chief Engineer, Treasurer, North American Iron & Steel Co., Inc., Brooklyn, N. Y.

Levine, Celia, M.A. Graduate Student, Department of Biochemistry. Columbia University, New York, N. Y.

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Pan, Shih-Yi, M.D. Instructor, Pharmacology, Columbia University, New York, N. Y.

Paul, Henry E., Ph.D. Head, Biological Division, Eaton Laboratories, Norwich, N. Y.

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Pope, Alfred, M.D. Research Associate, Neuropathology, McLean Hospital, Waverley, Mass.

Roesch, Philip K., M.A., Nutrition. Research Associate, Lederle Laboratories Division, American Cyanamid Co., Pearl River, N. Y.

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Schatz, Albert, Ph.D., Protistology. Assistant, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

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Stumbo, Charles R., Ph.D. Chief, Bacteriological Section, Central Research Department, Food Machinery Corp., San Jose, Calif.

Vollmer, Erwin P., Ph.D., Endocrinology, Epidemiology. Physiologist, Naval Medical Research Institute, Bethesda, Md.

Wells, Roe E., M.D. Pharmacologist, Medical Division, Army Chemical Center, Md.

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SECTION OF GEOLOGY AND MINERALOGY

TRIASSIC LIFE IN THE SOUTHWESTERN UNITED STATES

By EDWIN H. COLBERT*

Introduction. In the southwestern United States, particularly in western Texas, New Mexico, northern Arizona, and southern Utah, there are vast exposures of colorful rocks constituting some of the most extensives areas of continental Triassic sediments in the world. These Triassic beds form much of the striking and highly-colored scenery for which the southwestern states are justly famous, and for this reason they have for many years been of unusual interest to many people visiting this region. To the geologist, the Triassic rocks of the Southwest are significant because they afford substantial clues as to the history of continental North America during early Mesozoic times, while to the paleontologist these sediments are of especial importance because they contain numerous remains of plants and animals that inhabited the land through the Triassic period.

Various investigators, representing a number of scientific institutions, have worked in the Triassic of the Southwest and have collected fossils from this region. In recent years, the institutions most active in the paleontological study of the southwestern Triassic have been the University of California, the Museum of Northern Arizona, the University of Texas, the University of Chicago, Yale University, Harvard University, and The American Museum of Natural History. At the present time, an extended program of research on Triassic vertebrates is being carried on at this Museum of which the collection and study of fossils from the southwestern states form an integral part. This program, which is concerned particularly with the Triassic of Arizona and New Mexico, forms the subject of the present contribution.

The Sequence of Triassic Sediments in Arizona and New Mexico. In Arizona, the lower Triassic is represented by the Moenkopi formation, which may rest upon either the Kaibab limestone or the Coconino sandstone, of Permian age. Above the Moenkopi, and separated from it by an erosional unconformity, is the Shinarump conglomerate of upper Triassic age, a persistent and characteristic stratigraphic and topographic feature in Arizona, while above the Shinarump and conformable upon it is the upper Triassic Chinle formation, perhaps the most widely exposed of the Triassic formations in the Southwest. Finally, above the Chinle, in many parts of Arizona is the Wingate sandstone of

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Jurassic age. In some localities, there is a layer of sandstone between the typical Chinle and the typical Wingate, designated by Camp and his associates as the "orange red sandstone," the age and relationships of which are debatable. Some authorities would associate the orange red sandstone with the Chinle below; others with the Wingate above.

The lower Triassic generally is not present in New Mexico, so that the upper Triassic rests upon the Permian in this region, the earlier period being represented by the Abo, Yeso, and San Andres formations or by the Cutler formation. At the base of the upper Triassic in New Mexico, there are commonly found sandstones and shales, the Agua Zarca sandstone, the Salitral shale, and the Poleo Sandstone. These are followed by characteristic Chinle sediments of upper Triassic age. Finally, in New Mexico, as in many parts of Arizona, the Chinle is succeeded by the Wingate sandstone of Jurassic age.

Distinguishing Characters of the Triassic Sediments in Arizona and New Mexico. The Moenkopi is composed of dark red or brown sandstones, alternating with thin shales. The Moenkopi sandstones generally are hard and resistant, with the result that they frequently form prominent ledges or cliffs, and these exposures of brownish or dark red resistant rocks are very distinctive features of the landscape. In addition to the heavy sandstones and the thin shales there are, within the Moenkopi, lenses of gypsum and limestone beds of local extent. In eastern Arizona, the Moenkopi is rather thin, having a vertical extent of a few hundred feet, but to the west it thickens, so that in southern Utah it may attain a thickness of two thousand feet or more. In this region, there is an interfingering of marine sediments from the west with the Moenkopi.

Formerly, the Moenkopi was considered to be barren of fossils, but work within the past few years by the University of California has shown that this formation contains a fauna of early Triassic fishes, amphibians, and reptiles.

In Arizona, the Shinarump is composed of gray or brownish sandstones and conglomerates which form a persistent and prominent erosional feature, and because of this the Shinarump has been mapped as a separate formation. It is, however, the opinion of many students of the Triassic sequence in the Southwest that the Shinarump is no more than a basal conglomerate and sandstone, properly to be included within the Chinle formation. This seems to be a logical view, for there is no distinct unconformity separating the Shinarump from the overlying Chinle; rather, the one grades into the other. It is an interesting fact that the Moenkopi, the upper surface of which is quite uneven, locally may project up through the Shinarump to form a direct contact with the Chinle, an indication that the Shinarump was nothing more than a basal sandstone deposited upon the uneven Moenkopi surface, and of such a small thickness as not to cover completely this surface. The Shinarump, which often contains silicified wood, generally has a thickness of from fifty to one hundred feet.

In northern New Mexico, there are also basal sandstones at the base of the Chinle formation. These are the Agua Zarca and the Poleo sandstones, and, where both are present in one locality, which is not generally the case, they are separated from each other by the Salitral shale. Each of these members has a thickness of about fifty to one hundred feet. It has been suggested by Darton that the Poleo, a massive, gray sandstone in which there are plant remains, may be the equivalent of the Shinarump to the west. If this is the case, the same probably applies to the Agua Zarca and the Salitral as well.

The Chinle is the most widely exposed of the Triassic formations in the Southwest, and the great areas of which this formation forms the surface are marked by their striking colors and badland topography. The formation, which may range from four hundred feet or less in thickness to more than a thousand feet, is characterized by a varied array of brilliant colors—reds, pinks, grays, lavenders, purples, chocolates, and greens. In the Painted Desert region, the Chinle colors reach their most variegated and brilliant expression. Soft clays and bentonites are characteristic of the Chinle, especially in its lower parts, and because of this it is generally a weak formation, eroding into rounded badlands. There are, however, numerous local lenses of sandstone, and these are most abundant in the upper portions of the Chinle as exposed in north-central Arizona, in the vicinity of Cameron.

The Chinle has produced abundant fossils of plants and animals, for which reason it is well known to students of ancient life. In parts of Arizona, the Chinle contains great quantities of petrified wood, particularly well exemplified in the region of the Petrified Forest National Monument. The wood, in the form of large silicified logs and stumps, is closely related to the modern *Auracaria,* now limited to the Southern Hemisphere. In addition to the remains of these great trees, the Chinle contains other plants, notably impressions of fern leaves.

Local deposits of unionid shells are common in the Chinle, indicative of the presence of river channels and shoals, in which extensive mussel beds were accumulated. The vertebrates, and especially the land-living vertebrates are, however, the most characteristic fossil animals of Chinle age. The fauna contains certain fresh-water fishes, such as might be expected in stream channels or small lakes, stereospondyl labyrinthodont amphibians, and various reptiles, notably phytosaurs, crocodile-like thecodonts that must have been ubiquitous and dominant during Chinle times, *Typothorax,* an armored pseudosuchian, *Placerias,* a dicynodont therapsid, and *Coelophysis,* a primitive theropod dinosaur.

TABLE 1

	Utah Arizona	New Mexico	Texas	Wyoming	Eastern U. S.
Jurassic	Wingate	Wingate			
	(Orange red sandstone)				
Triassic	Chinle	Chinle			Newark Series
			Dockum	Popo Agie	
	Shinarump	Poleco Salitral Agua Zarca			
	Unconformity				
	Moenkopi	Unconformity			
	Unconformity				
Permian	Kaibab	San Andres			
	Coconino	Yeso Cutler			
		Abo			

In northern Arizona, in the vicinity of Cameron, the orange red sandstone immediately above the Chinle and below the typical Wingate can be regarded as either of Triassic or of Jurassic age, and valid arguments can be advanced for either assignment. It was in this horizon that The American Museum of Natural History discovered, some years ago, the complete skeleton of an ancestral crocodilian, *Protosuchus*.

Correlation of the Continental Triassic Beds of North America. The lower Triassic Moenkopi probably has no real equivalent in North America. The upper Triassic Chinle, on the other hand, can be correlated in an approximate fashion with the Dockum of Texas, the Popo Agie of Wyoming, and the Newark Series of eastern North America. Exact correlations are not possible, but it would seem probable that the Dockum and the Popo Agie are equivalent to only a portion of the Chinle, probably the lower part. The vertical extent of the Newark series is a difficult question; it is probably equivalent to the entire Chinle sequence and possibly covers even more geologic time than does the Chinle formation.

The relationships of the Triassic formations of the Southwest to those of other parts of the continent can be represented as shown in TABLE 1.

The Triassic Faunas of Arizona and New Mexico. Turning, now, to a brief consideration of the fossil animals discovered in the Triassic sediments of Arizona and New Mexico, the following list is here presented, showing the stratigraphic sequence of genera.

Orange Red Sandstone
- Reptiles
  - Saurischia
    - Carnivorous dinosaur
  - Crocodilia
    - *Protosuchus*—ancestral crocodilian

Chinle Formation
- Reptiles
  - Saurischia
    - *Coelophysis*—primitive theropod dinosaur
  - Thecodontia
    - *Machaeroprosopus*—phytosaur
    - *Typothorax*—armored pseudosuchian
    - *Episcoposaurus*—pseudosuchian
  - Therapsida
    - *Placerias*—dicynodont
- Amphibians
  - Labyrinthodontia
    - *Buettneria*—stereospondyl
- Fishes
  - Holostei
    - *Semionotus*
    - *Lepidotus*
    - }—"ganoids"
  - Dipnoi
    - *Ceratodus*—lung fish
- Mollusca
  - Pelecypoda
    - *Unio*

(*Shinarump Conglomerate*)
- Fragments of phytosaur and amphibian bones
- Paleozoic invertebrates, secondarily deposited

Moenkopi Formation
- Reptiles
  - Thecodontia
    - *Arizonasaurus*
    - *Chirotherium*
    - }—pseudosuchians
  - Incertae sedis
    - *Anisodontosaurus*
- Amphibians
  - Labyrinthodontia
    - *Taphrognathus*
    - *Cyclotosaurus*
    - *Rhadalognathus*
    - *Aphaneramma?*
    - *Capitosaurus?*
    - }—stereospondyls
- Fishes
  - Subholostei
    - *Boreosomus?*—"ganoid"
  - Incertae sedis
    - *Leiacanthus*

Triassic Life of the Chinle Formation. The most common fossil vertebrate in the Chinle formation is the phytosaur, *Machaeroprosopus*. To judge by the evidence of the fossils, this reptile must have been widely distributed and very numerous during upper Triassic times, and it certainly must have been one of the dominant animals in the Chinle fauna. Of course, it should be remembered that the phytosaurs, being water-loving animals, lived in an environment where their bones were very likely to be fossilized, so that the frequency of phytosaur remains as compared with other Chinle reptile fossils may be misleading to some degree. Nevertheless, there can be no doubt but that phytosaurs were very abundant in the Chinle rivers and swamps; indeed, their occurrence must have been similar to that of crocodilians along modern tropical rivers.

The phytosaurs played virtually the same role in Chinle times that the crocodilians do today. They were the aggressive, semi-aquatic predators that haunted rivers and swamps, to feed upon fish and any large animals that might come to the water to drink. It must not be thought, however, that the phytosaurs were ancestral to the crocodilians, because they represent a distinct evolutionary line that went through the course of its phylogenetic development and became extinct before the crocodilians arose in Mesozoic times. This offers a prime example of parallelism in evolution, of the similar development of two groups which, while unrelated, were nevertheless descended from a common ancestry.

This brings up a question. Why did the phytosaurs become extinct at the end of the Triassic period? Why should they fail, while the crocodilians, which succeeded them and imitated them so completely, were successful over a tremendously long sequence of geologic time ranging from the beginning of the Jurassic to the present day? Phytosaurs were distributed over North America and Europe during the upper Triassic, and seemingly they were very successful animals at that time. But at the end of the Triassic, they disappeared completely, and the crocodilians evolved to assume the same ecological niche that had been filled by the phytosaurs.

Typothorax and *Episcoposaurus,* pseudosuchians, were related to the phytosaur, *Machaeroprosopus.* Fossils of these two genera are not common, and it is possible that this may be due in part to the fact that these were upland reptiles, spending much of their time away from the immediate proximity of the water. They were heavily armored animals, and *Typothorax* attained a rather large size. Evidently, their very heavy armor protected these animals against attack by the phytosaurs.

A recent discovery by The American Museum of Natural History has unearthed, in northern New Mexico, abundant remains of a small, primitive theropod dinosaur, *Coelophysis.* This was a rather fragile little animal, six feet or so in length and lightly built, so that in life it probably weighed no more than forty or fifty pounds. Like all theropod dinosaurs, *Coelophysis* was bipedal; the hind limbs were used for locomotion and the front legs, which were reduced in size, were utilized for grasping food. The teeth, though small, were very sharp, and it is evident that this little dinosaur preyed upon small reptiles of various sorts. It was a quick, active little predator, viciously hunting its prey and keeping out of the way of larger reptiles by virtue of its agility and speed.

Heretofore, *Coelophysis* had been known from very incomplete material, but as a result of the recent discoveries by The American Museum of Natural History we shall now have abundant evidence as to the anatomy of these early dinosaurs. Numerous completely articulated skeletons were found piled on top of one another. What is the explanation of such an occurrence? What forces of extermination and deposition should bring together in one small spot so many beautifully preserved specimens? The deposit is remarkable, not only for the abundance of the bone and the articulation of the skeletons, but also because of the complete preservation, even down to the tiniest skeletal elements. It would seem to be a stream channel deposit, but conditions must have been unusual, to say the least, to account for this remarkable preservation of fossils.

Coelophysis probably was an upland animal, and the same was likely true of the therapsid reptile, *Placerias,* a large dicynodont reptile, closely related to the dicynodonts so typical of the Karroo series in South Africa. The discovery of complete *Placerias* material in Arizona by the University of California indicates that this reptile was a characteristic element in the Chinle fauna. Also, it adds to our knowledge of the distribution of the dicynodonts, which now are known from all of the continental areas. These obviously were widely distributed reptiles in Permian and Triassic times.

In contrast with the upland reptiles of the Chinle, the amphibian, *Buettneria,* was an inhabitant of rivers, lakes, and swamps. It was a large, stereospondyl labyrinthodont that secondarily had become almost completely adapted to life in the water. The skull was very flat and heavily ossified, as were the elements of the dermal shoulder girdle, but much of the remainder of the skeleton must have been cartilaginous, because bones other than the skull and the shoulder girdle elements are not common in the Chinle sediments.

Living in the streams and lakes with *Buettneria* were certain fresh-water fishes, notably the holosteans *Semionotus* and *Lepidotus,* ganoids with heavily armored scales, and the lungfish, *Ceratodus.* The presence of *Ceratodus* in the Chinle sediments is very interesting, since it gives us a good clue as to ecological conditions of that time. *Ceratodus* is closely related to the modern Australian lungfish, *Epiceratodus.* This modern lungfish lives in small pools that become much reduced during the dry season so that the water gets very foul. At such times, the lungfish is able to come to the surface to breathe air, and thus to survive in an environment inimical to most other fishes. It is logical, therefore, to assume that, during Chinle times, there were many small and stagnant pools, in which the amphibians and the lungfish were able to live.

The aquatic facies of the Chinle sediments are characterized by occasional deposits of unionid shells, which in some localities form extensive beds containing vast quantities of fossil molluscs. At one place in the Petrified Forest region there is such a deposit, where the shells weather out in countless thousands, forming a desert pavement over the slopes of the badlands. It seems quite obvious that deposits such as these indicate extensive mussel shoals in the rivers of Chinle times, very similar to the mussel beds that are found in present-day rivers and streams.

From this brief discussion of the life of the Chinle, it is possible to draw a picture of the ecological and faunal relationships that held during upper Triassic times in the southwestern United States. Evidently, this region was a low-lying flood plain, across which there meandered sluggish streams. To the west was the sea. It was a warm, tropical or subtropical land that supported an abundant plant and animal life. The flat expanses of the flood plain were interrupted here and there by volcanoes, which periodically ejected ash in great quantities. This volcanic activity, together with seasonal flooding and drying up of the rivers, resulted in the deposition of bentonites and stream channel sands, in which the plants and animals that are now preserved as fossils were entombed.

On the low uplands, away from the rivers, were forests of auracaria-like trees, while along the stream courses were giant horsetails, ferns, and other lush vegetation. Living in the woodlands and along the green bordered rivers were the various tetrapods of that distant age, while in the waters of the rivers and lakes were certain aquatic tetrapods and fishes.

The giant dicynodont, *Placerias,* was characteristic of the upland surfaces, as was the little primitive dinosaur, *Coelophysis.* The same habitat may have been the haunt of the armored pseudosuchians, such as *Typothorax.* It must not be thought, of course, that these animals were limited to the more upland areas, because it is evident that they also frequented the banks of the rivers. Here, however, life was dangerous for them, because of the presence of the aggressive, crocodile-like phytosaur, *Machaeroprosopus.* Sharing this ecological zone with the phytosaur, and perhaps competing with it to some extent, was the large labyrinthodont amphibian, *Buettneria.* It is probable that both the phytosaur and the amphibian fed upon fishes, so it is not surprising to find that the freshwater fishes of the Chinle were well-protected by heavily armored scales. The fishes were holosteans, or "ganoids," and lungfishes—well adapted to life in waters subjected to periodic fluctuations.

Triassic Life of the Moenkopi Formation. The fauna of the Moenkopi, as based upon present knowledge, is neither so extensive nor so varied as that of the Chinle. Most of the tetrapod fauna consists of stereospondyl amphibians, which would indicate a prevalence of streams and swamps on a flood plain. The reptiles are known from incomplete materials, which indicate pseudosuchians of rather small size. The fishes are even more incompletely known, and such fragments as have been found show us that these were mainly holosteans with heavy scales.

On the whole, the picture of life in Moenkopi times is essentially similar to the picture of Chinle life that already has been drawn, and it is probable that, if the Moenkopi vertebrates were more completely known, the similarities would be increased.

Conclusion. Such is the story of life during Triassic times in the southwestern United States. It was a period of great importance in the evolution of the vertebrates, and especially of the land-living tetrapods. This was a time when amphibians and reptiles were in a state of change, when certain old lines were still holding on from earlier periods of dominance, when various new lines were still in the initial stages of their adaptive radiation. Thus, we see the labyrinthodonts in the streams and the dicynodont reptiles on the land as relicts of groups that were especially characteristic of the preceding Permian period. Peculiar to this stage in earth history was the dominance of the thecodont reptiles, the phytosaurs and the pseudosuchians. These were new lines of phylogenetic development that had their beginnings with the initiation of Mesozoic history, yet which were destined to die out at the end of Triassic times. Why these thecodonts, seemingly so well adapted and so successful during the Triassic, should not have survived the transition from the Triassic to the Jurassic period is one of those mysteries of extinction that so frequently occur throughout the extent of geologic history. Finally, in the Triassic of the Southwest, we now have as the result of recent work abundant evidence regarding the primitive theropod dinosaurs. This group, having its beginnings in the Triassic period, was to prove eminently successful during succeeding geologic ages. From small theropods, of which the Chinle genus *Coelophysis* is typical, there sprang the various lines of saurischian adaptive radiation, lines that were to persist through 100 million years of Jurassic and Cretaceous history, thereby establishing themselves as among the most successful of the land-living vertebrates.

GROUND WATER IN THE NEJD, SAUDI ARABIA[*]

By RICHARD BRAMKAMP[†] AND GLEN F. BROWN[‡]

The Nejd, Saudi Arabia, is near the center of one of the world's largest deserts, where economy is in delicate balance with water supply. Recent agricultural development requires expansion of the dominant supply derived from subterranean sources—wells in *wadi* floors and pits formed by solution-stoping of anhydrite and limestone.

The western third of the Arabian peninsula is occupied by a shield or nucleus known as the Nubian-Arabian massif around which are deposited sedimentary rocks ranging in age from early Paleozoic to Miocene. These beds form long arcuate cuestas emphasized by the variable nature of the sediments and the long history of wind erosion, the limestones forming topographic highs, the arenaceous beds forming lowlands and plains. The most important limestone cuesta group is the Tuwaiq mountains which extend from about latitude 19° north to 27° north along a curved path 1200 kilometers long. An extensive lowland on the west side of these limestone mountains is underlain by a non-marine sandstone, equivalent to the lower Mesozoic Nubian type of Africa. The Tuwaiq mountains are composed of several hundred meters of limestone, dolomite, and minor shale which dip eastward toward the Persian Gulf and include a bed of anhydrite in the upper part of the stratigraphic section. They are of lower Cretaceous and older Mesozoic age. Above them lies another important non-marine sandstone of middle Cretaceous age which forms the stretches of the white gravel plains on the east side of the southern Tuwaiq mountains.

Rainfall is scanty, probably not exceeding four inches per year, and of the desert torrential type. It has been sufficient to maintain three valleys across the Tuwaiq mountains in the area between Wadi Dawasir and Riyadh. Water is found in the gravels of the *wadis* after the winter rainy season of some years. The best water, lowest in total dissolved solids, is in those *wadis* having steepest gradients or those located nearest to uplands where evaporation and salt accumulation have been least active.

The trans-Tuwaiq *wadis* converge at Al Kharj, about 100 kilometers south of Riyadh, and the drainage line extends east as Wadi Sahaba. This confluence seems to be due to an antecedent drainage pattern developed on the arcuate pattern of the Tuwaiq table-land, the subsequent tributary drainage along the western sides of the cuestas, and a narrow graben, Shaib Nisa, which cuts through the Tuwaiq mountains directly west of Al Kharj. This graben was first noted by Mr. Max Steineke and subsequently studied in detail by Dr. Bramkamp. The latest work led to two other subsidiary grabens on the north of Shaib Nisa, neither of which is sufficiently active to control drainage except locally.

Movement of water eastward in this drainage pattern has dissolved anhydrite which lies beneath a remarkable cliff, the Hitt escarpment, extending from near Riyadh in the north to the vicinity of Laila in the south. Water in contact with the calcium sulfate is nearly saturated by the salt, and solution and collapse of the terrain in a belt about 12 kilometers wide on the west side of the Hitt escarpment have formed a valley by subtraction from below. The solution of anhydrite and the retreat of the Hitt escarpment eastward have been a function of time since Mesozoic and of sufficient ground-water gradient to remove water saturated with calcium sulfate from the anhydrite face. There is much evidence that periods of rapid solution have been separated by long periods of quiescence. During one of the quiescent periods, the collapsed zone was submerged beneath a large fresh-water lake. Subsequently, fault movements on the eastern end of the Nisa trough started subterranean water circulation anew, removed anhydrite, and caused the Hitt escarpment to move east to its present position. In places, it appears to have moved about two or three kilometers in the last cycle. Locally, the water has dissolved out sufficient material to cause stoping through to the surface at the base of the cliff. Three of these pits, about 100 meters deep, are on the south side of the valley at Al Kharj, where the water is being pumped for irrigation purposes in the Kharj district and east onto the flood plain of Wadi Sahaba. The confined water of the calcium sulfate zone has spilled over into the *wadi* gravels, raising the flank of the normal gravel water table and causing evaporation and salt accumulation in the lowest part of the Kharj *wadi* floor.

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[*] Published with permission of the Arabian-American Oil Company and the Director, U. S. Geological Survey.

[†] Arabian-American Oil Company, Dhahran, Saudi Arabia.

[‡] U. S. Geological Survey and Department of Geology, Northwestern University, Evanston, Ill. This paper, illustrated by lantern slides, was presented by Mr. Brown at the meeting of the Section on February 2, 1948.

Note. The Section of Biology held a meeting on April 12, 1948, at which Mr. Fairfield Osborn, President, New York Zoological Society, presented a paper entitled *The Planet and the Plunderer,* which was illustrated by motion pictures. No abstract of this paper has been received.

SECTION OF PSYCHOLOGY

PRESENT STATUS OF DEVELOPMENTAL AND REMEDIAL READING PROGRAMS AT THE COLLEGE LEVEL

By FRANCES ORALIND TRIGGS*

It has been said that every educator is, at heart, a missionary. I wonder if most readers would agree with this. I consider myself, first, a psychologist with special training in the field of reading, but in a larger sense I am first an educator. As such, I am sold on the thesis that, once we educators and psychologists understand the importance of reading as a tool, first, to gain personal breadth and adjustment, and, second, to gain understanding of personal and social responsibilities, we shall make great strides forward in aiding every student who passes through our educational system to attain reading skills to a much larger extent than we are achieving now. Therefore, I welcome the opportunity to discuss this subject. I have in mind a specific way in which I would like to present the subject and certain thoughts that I would like to leave which, if followed, I hope will affect action on the part of those in a position to develop reading programs in college.

The recognition of reading as a skill important to learning, especially in a society which governs itself, has been given careful consideration. Probably, the yearbooks of the National Society for the Study of Education are the most adequate record of this consideration, especially the Twenty-Fourth, the Thirty-Seventh, and the Forty-Seventh. To date the Twenty-Fourth Yearbook has sold 40,000 copies—a record even for a book of fiction. It was not until the last yearbook, the Forty-Seventh, which appeared in February, 1948, that major recognition was given to the fact that the development of these skills does not end with high school graduation. Its title is *Reading in High School and College.* Its theme is that reading as a technique of learning and self-development can be greatly improved even after students get to college; that many students will not be successful in gaining the understandings presented to them as they go through college, unless they receive careful help in developing their reading skills after they enter college.

In the present paper, I hope to show why there is the task before every college of consciously developing a reading program. I also hope to show how this can be done. Reading instruction in college must be, at the present time and for some years to come, of both a remedial and a developmental nature. The remedial or corrective phase should usually be handled by a specialist in the field of reading who is also a student counsellor or clinical psychologist. This work is needed by students who have not, up to the time of their entrance into college, developed what may be called the basic reading skills. The developmental program is best handled by each faculty member as he aids his students to achieve efficient learning in his specialized field. In this, the faculty member often calls on the reading specialist for suggestions and techniques, but ideally the developmental reading program itself will be in the hands of the professors teaching specialized subject matter, such as history, mathematics, English, psychology, etc. Reasons for this will be given later.

Can we define what the professor needs to know of reading, and learning to read, in order to aid students to utilize efficiently the specialized reading skills most necessary for efficient instruction in his subject matter field? I would mention the following*:

He needs an understanding of (1) the nature of reading as a psychological process; (2) the factors which contribute to reading difficulties and deficiencies; (3) the factors inherent in the curriculum materials he is presenting to the students which cause reading difficulties; (4) the relationship of reading to other learning tools such as visual aids; (5) the techniques for evaluating the extent to which students have attained or are attaining the reading skills necessary for success in the specialized subject matter in which he is teaching; and (6) the sources in the professional literature on reading to which he may turn when a so-called "reading specialist" is not at hand to give the help he needs. It is obviously impossible for us to approach or mention all of these subjects here, but there are two which I should like to review especially: the nature of the reading process, and the specialized skills needed for efficient learning in a few of the subject matter fields.

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* Committee on Diagnostic Reading Tests, Inc., New York, N. Y. This paper was presented at the meeting of the Section on April 19, 1948.

* This topic is also discussed in the Forty-Seventh Yearbook of the National Society for the Study of Education.

Reading, narrowly defined, is largely a perceptual act—specifically, visual perception—that process in which the individual's reactions are related to the printed page as the stimulus. It is a truism to say that, if there is no reaction on the part of the subject, there is no reading. Some specialists leave the situation pretty much at that, while others feel that reading is much more than that. However, one of the first principles every person working in the field should recognize is that the reader must master the perceptual act of reading. As long as his reaction to the stimulus of the printed matter is delayed by the perceptual act, he will never be an efficient reader and there are college students and adults who have never gotten beyond this stage in their development. This obvious fact is often assumed because other persons, having mastered these skills themselves, are unaware that such problems exist for some. What are the related terms utilized when this area is referred to? A few we may mention are word attack, word recognition, and the mechanics of reading. However, reading itself is a much more complicated act than the term, perception, may imply, as we shall see when we come to define the reading tasks of college students.

Before we discuss the specialized reading skills needed for reading in the content fields, it would be well for us to mention reading skills which must be developed. It should not be felt that this description is one agreed upon by all specialists in the field. Disagreement exists, and I can only give an explanation, not a scientific description.

The skills which might be mentioned can be classified under five main headings:

(1) *Word recognition,* largely based on the perceptual act already discussed. This involves being able to recognize words from total configuration and noting differences in words of like configuration by dividing words into syllables, ability to sound units of words such as syllables, at the same time recalling meaning from context, and using spoken vocabulary as a clue to meaning; recognition and recall of meaning of prefixes and suffixes, and root words; and, to some extent, grammatical structure. These skills are often referred to as the mechanical reading skills.

(2) *Vocabulary* (again based on word recognition and overlapping with the skills just described under that heading). This means vocabulary including deduction of meaning from context, knowledge of word parts, use of dictionary; appreciation of connotation, including word relationships, synonyms, antonyms, idioms, figurative or relational expressions; word history (etymology); word usage; understanding of symbolic expression, including maps, charts and diagrams, graphs, formulae, tables, signs, symbols, and relational expression.

(3) *Rates of reading skills,* including oral reading; skimming—to see whether the material satisfies one's purpose (*i.e.,* to get the gist of the content quickly) by noting chapter titles, marginal and other subheadings, topic and transition sen-

tences, key words, "signposts" (*e.g.*, therefore, however, and series words like first, second, third); rapid but thorough reading (the ability to combine speed and thorough comprehension); intensive reading —to trace the solution of a problem; ability to adapt reading speed to the reader's purpose, to the author's purpose, or to the type of material.

(4) *Skills used in apprehending meaning.* Ability to organize material including recognizing and formulating the central idea, finding details to support the central idea, seeing details in proper relationship, arranging events in sequence; ability to remember what is read, including reading with intention to remember, ability to concentrate, knowledge of helpful specific skills such as outlining, notetaking, underlining important points, making marginal notes, summarization by units, meditating on and discussing the material covered; ability to evaluate and appraise the material read (*i.e.*, critical reading), such as ability to locate supplementary information, including use of library catalogs, of periodical and book indexes, of other reference tools; ability to think logically (abstract thinking), such as to suspend judgment, to see the relation of the material read to reader's background and experience, time, and setting; to draw conclusions; to judge the author's performance by checking the validity and reliability of his sources, checking the accuracy of the author's inferences in the light of the material presented; ability to act on the basis of what has been read by following written directions, answering questions requiring reasoning.

(5) *Reading for appreciation.* Ability to visualize the situations and places described, to respond emotionally to the material read, and to appreciate tone and rhythm (especially in oral reading and poetry).

This classification, arranged roughly according to the complexity of the skills, is not meant to imply that the various skills are rigid, independent units within the reading process. They are all closely interrelated, and for this reason the diagnosis of reading difficulties can seldom be exact. It can show us the general area of the difficulty, and, from there on, remedial work must concentrate on improving all deficient skills included in that area.

Before discussing this, however, we should remind ourselves that, over and beyond the task of visual perception itself, reading requires the integration of thought processes stimulated by the perceptual act. Such psychological phenomena as attending, concentrating, remembering, recognizing the writer's purpose, the reader's purpose, the type of material with its specialized reading problems, are all related. An efficient reader uses these phenomena, more or less consciously, while the poor reader is likely to fumble through them largely unconscious of the fact that they exist.

The reading act, then, varies not only from person to person but from subject to subject, and, to complicate matters further, it varies with the individual in accordance with his purposes for reading, momentary as these may be at times.

Before discussing the reading tasks the college student faces and going farther into reading deficiencies which college students are found to have, I should like to tarry on two subjects: the problems readers meet in approaching reading in various subject matter fields, and the types of problems found in various college curricular areas. Perhaps this discussion will give some suggestions as to how some of these problems may be met.

The Reading of Prose Literature. We have heard the saying that the purpose both of the writer in writing and of the reader must be considered when reading. This is especially important in the reading of literature. The same piece of literature may not only be written for a number of purposes; it may be read to meet various types of needs of the reader. Possible author's purposes may be to record historical facts, to tell a story, to affect social conditions, etc. The reader must not only learn to recognize the author's purpose but must also define his own, for he will utilize skills depending to some extent upon his own purpose for reading.

Literature is certainly one type of reading which affects personal development. Therefore, the aspect of the reader's purpose and his ability to choose material pertinent to his needs becomes of paramount importance. Also, the reader, if efficient, will vary the rate at which he reads material according to his need. His general conceptual background as well as the extent to which the vocabulary used is familiar to him will, to a large extent, be factors which will affect the rate at which he reads.

The rate at which literary material is read will also differ with the form of the literature. Drama, for instance, will usually be read more slowly than narrative form because visualization of actions normally intended to accompany the spoken word, with background supplied, must take place or much of its value is lost.

There is literature so written that its meaning is obvious on the surface, but also that which carries hidden meanings. If, by any chance, these meanings are similar to rich, emotional experiences of the reader, and the reading causes the reader to recall those experiences, the matter becomes even more complicated.

The matter of vocabulary difficulty has already been mentioned.

The reading of newspapers is perhaps our best example of the need for critical reading. Many students enter college with blind faith for anything they see in print. When they first meet two exactly opposite statements made in print, they become confused and unable to apply the normal "checking" skills to draw conclusions for themselves. Far too often, for lack of this skill, reading is not the growth experience it should be. The reader discards one or the other idea without any real reason for doing so, usually according to his previous bias. Perhaps part of each is true! Constant evaluation is necessary in certain types of reading if the reader is to gain.

Reading in the Social Studies. The first reading problem in this field is undoubtedly related to concepts suggested by the vocabulary used. True, some new and strange words will appear but, more important, words in strange connotations will come up. Metaphorical expressions are numerous. Examples might be "He can be depended on—Minnesota produces hardy stock" or "The real purpose of the Marshall Plan is pump-priming."

Included in this general problem are concepts of time and place. Such terms as sixteenth-century, ancestral homeland, a generation or so ago, must be given some reality through experience before they can be considered meaningful, though the words themselves are not difficult.

Methods of presenting quantitative data in the social studies are often strange and new to readers. Graphs, charts, maps of various kinds actually require a process of translation. The pictorial method of presenting data, instead of making this problem simpler, has actually complicated it to some extent. One has to go through an additional process before he gets the facts to translate. True, pictorial graphs may cause the readers to attend to them when otherwise many readers would overlook charts and graphs entirely.

The matter of critical reading, discussed earlier in newspaper reading, comes up again in reading of the social studies. It is a matter for real concern when, in one class, students may spend a semester learning to form concepts based on what they thought to be adequate facts without ever considering the other side of the problem. If those concepts are based on current happenings, they must learn to hold their conclusions in abeyance while the facts are tested by time. How can a student, when reading, determine the adequacy of the data presented? He must be shown. All propaganda is not bad, but students must be able to judge when material being presented to them falls in this area. They do so by critically weighing material as they read, holding in their minds the meanings gained from reading until they can weigh and test conclusions drawn.

Reading in Mathematics and Science. In the chapter which discusses the reading of mathematical material in the Forty-Seventh Yearbook of the National Society for the Study of Education, *Reading in High School and College,* four major reading problems are covered: first, the language of mathematics, which includes literal numbers, operational symbols, arithmetical numbers, and expressions of relationships; second, the vocabulary of mathematics; third, obtaining meaning from symbols; and, fourth, problems relating to the compactness of the writing.

These problems, to some degree, are the same problems which arise in reading science material. To overcome them, the student must know how to adapt his reading speed to the point where he gets most efficient comprehension. He must be able to translate into his own words the symbolic language, and he must enrich the material read from his own experience because of compactness with which it is written.

The Reading Deficiencies of College Students. When reading disabilities of students are mentioned, the one which students coming to the clinic most frequently identify and complain about is reading too slowly. Too often, rate of reading is identified as an entity in and of itself unrelated to the purpose of the author, the reader, or the type of material being read. The job of the reading clinician is to determine the reason or reasons for slow reading: inadequate vocabulary, habitual slow reading, poor perceptual or recognition skills, and poor concentration often due to problems of adjustment. Remedial work depends on the diagnosis.

Inflexibility of reading skills is closely related to slow rate of reading. An illustration might be given. A student given a reading list from which to select supplementary reading on a topic to be developed for class discussion must be able to skim the references to determine whether they are pertinent to his needs. It may be found that the material is pertinent, but essentially covered by other references. Usually, it must be quickly read anyhow to be sure that no new content is included. All of this adaptation requires flexibility of skills.

Some students do not know how to attack words not already in their reading vocabulary. Therefore, if new words are contained in the material read, the student must master them before he can read with meaning. Reading of formulae, charts, graphs, and other methods of presenting data must be mastered also. Critical reading, as already mentioned, is a problem for some students. All students, even though their basic reading skills are good, must learn to adapt their skills to the specific content they are reading.

All of the difficulties mentioned above stand in the way of efficient critical reading. In order to read critically, a reader must bring something to his reading from his experience and previous reading. He must also know why he is reading and apply the appropriate skills for the purpose. There is very little of any experience in life which can be accepted uncritically. This students fail to understand as often as not, and many graduate from college with no appreciation of that fact at all. Many colleges assume students knew it when they entered college. As a result, they never learn the importance of critical reading and critical thinking. A number of special techniques have been developed to bring this forcefully to the student's attention in the area of college reading assignments.

The emotional problems which are in some way related to reading disabilities of any identifiable magnitude at all cannot be overlooked if successful remedial work is to be done. Also, reading as a therapeutic technique, including bibliotherapy, is a very interesting and useful tool in the hands of a trained clinician.

While this list of disabilities is not exhaustive, it is illustrative of the deficiencies college students are found to have.

The Reading Specialist in the College Reading Program. We have mentioned the part the professor may play in advancing the development of reading skills of college students. What can the specialist in reading do to aid a college in setting up a reading program? He can work with the faculty members in individual departments, helping them to identify the specific reading problems in their curricular fields and suggest techniques which may be used with groups or in regular classes, as well as ways of evaluating the results of the work. He can also aid departments by handling the problems of individual students referred to him by the faculties of the departments, who do not respond to the group or class work.

To do a good job in work with individuals, he must, as the Yearbook suggests, combine the qualities of an educator, clinical psychologist, therapist, and an expert teacher. He must have a broad understanding of the mental, physical, and emotional growth of young people and a thorough knowledge of the psychology, physiology, and sociology of reading, for one of his jobs is to identify, diagnose, and treat problem cases in reading through a formally conducted reading clinic. To do this involves the treating of all the usual emotional problems which may be the cause of reading problems, the result of them, or both. The remedial work is, therefore, also affected by them.

The College Reading Program. How should colleges interested in developing better reading habits and skills approach the task? There is no one answer, but the interest of the entire faculty and administration must be elicited. The major part of any really successful program will be carried out by the faculty. This may be done by first furnishing expert help to the faculty through a reading specialist. The faculty members will refer to the specialist students who are obviously having trouble, and will get help from the specialist on reading techniques they can use in the classroom. The reading clinic, administered by the specialist, will handle the individual students referred for special help. To be of most help to those students, the reading specialist will need various types of help from the faculties of the various departments, both on general approach and on the work with individual students. The evaluation of the program should not be overlooked. Such evaluation will again be carried on jointly by the faculty, and the total reading program will be modified on the basis of the findings.

To summarize, then, it is my belief that, if colleges are to do their job today efficiently and well, they must give more attention than they are doing at present to the development of reading skills to a more efficient level than their students have when they enter college. To do this, it would probably be necessary for the present to have a remedial program, which should be in the hands of a trained clinician. This clinician will work with students to develop efficient basic reading skills. The larger job of developing students' skills will be done through the developmental program carried on in the class room. The specialist in reading will be able to help the college in a number of ways to establish and maintain a reading program, which, therefore, would not just be something added to the work of already overburdened professors. It is a way, actually, of relieving them of a problem they now face—students who do not read efficiently enough to profit from college work —and thus to remedy a situation in which neither the professors nor the students do their best work.

I should like to close by mentioning again the social implications of reading.

We proudly call our form of government democratic. This implies government of and by the people. No such government can stand without the active interest of every citizen, which means educated citizens, and no one remains educated without continual effort on his part. Reading is the one most important tool in our present-day society for our continued education as citizens after we complete our formal schooling. In colleges, today, we are not presumptuous enough to think that we teach students what they need to know throughout life. We hope, however, that they do gain resources such that they can continue to learn in order to carry their social responsibilities and to continue their personal and social development. Reading is, I think, the greatest avenue to learning today. Radio and motion pictures are vieing with it for their places as avenues of learning, but as yet they cannot begin to compete. If the individual citizen can read efficiently, there is almost no end to the possibilities of using that skill as a cheap and effective avenue of learning and enjoyment. As I said in the beginning, when we, as educators, are convinced of this fact, we shall begin to place the stress we should on the development of efficient reading skills in college. We shall not feel that our job is finished until students have attained efficiency in reading to the extent that the adult population, or at least that portion of it which graduates from college, is a reading public—a state of affairs which all research in this area has indicated not to be prevalent at this time.

Editor's Newsnote

Doctor John E. Vance, associate professor of chemistry at Yale University, has been appointed chairman of the Chemistry Department at Washington Square College and head of the Chemistry Department at the Graduate School of Arts and Sciences, New York University. He will assume his new duties with the opening of the Summer Session.

Professor Vance received his doctorate from Yale in 1929 and subsequently studied at the University of Copenhagen under a Sterling Fellowship. Upon his return, he joined the teaching staff at Yale. He entered the Chemical Warfare Service in 1942 and was transferred to the Manhattan District of the Corps of Engineers; among other missions, he was sent to Bikini as a technical adviser.

After returning to Yale in 1946, Professor Vance was appointed to the staff of the United Nations Atomic Energy Commission. Besides being a fellow of the Academy, he is also a member of the American Chemical Society and of Sigma Xi.

—Information supplied by the New York University Bureau of Public Information

SECTION OF ANTHROPOLOGY

THE PASTORAL PEOPLES OF NORTHWESTERN KENYA

By KEPLER LEWIS*

One year ago (April 10, 1947), the Morden African Expedition sailed from New York for British East Africa. One of its major objects was to make an ethnographical collection from an East African tribe for the sponsoring institution, The American Museum of Natural History. From this side of the Atlantic, it was difficult to decide on a tribe. We wanted a group which had not been too much acculturated and one which would not duplicate collections already in the museum.

A little research after our arrival in Nairobi indicated a tribe in northwestern Kenya Colony, the Turkana, as ideal for our purposes. The Turkana are an isolated people with little European contact. Their country is too arid to attract farmers and ranchers, and the natives had hunted so thoroughly that most of the big game had been cleared out before African hunting became an international sport, so that they were spared the many game safaris which still penetrate the Northern Frontier Province east of Lake Rudolf.

As far as we know, the first Europeans to enter this part of Africa were Count Teleki and Captain von Höhnel, who came around the south end of Lake Rudolf 63 years ago. They were impressed by the astonishing physiques of the natives and by the fact that they were such a lively, noisy and impetuous people. In 1896, Vittorio Bottego explored some of the area west of Lake Rudolf. He was followed by H. S. H. Cavendish and Major H. H. Austin, but the country never became a popular corridor for traffic. It did not come under British civil administration until after the first World War, and until 1921 there was an official Ethiopian governor of Northern Turkana. As late as 1930, there had been no general topographical survey of the area.

The eastern boundary of northwestern Kenya is well marked by Lake Rudolf, about 180 miles long and some 20 miles wide, and by the Samburu Escarpment, the eastern wall of the Great Rift Valley. The western boundary is formed in the north by the Uganda Escarpment and further south by an arbitrary line dividing Kenya and Uganda. The northern boundary line with the Anglo-Egyptian Sudan is based on neither geography nor ethnic distribution.

The highlands which mark the southern border of the region range from about 5,000 feet to nearly 11,000 feet in elevation and have sufficient rainfall for subtropical agriculture. There are deeply eroded valleys, mountain streams, and water falls. The cover varies from grass to thick bush on the hills and forest along the streams.

The west wall of the Rift Valley towers nearly 2,500 feet above the plains on its floor. From the base of the escarpment, these plains slope gently downward toward Lake Rudolf, which stands at just under 1,300 feet elevation. The plains vary from arid sandy steppe country to true desert. There are many minor streams winding through the country, but nearly all of them are dry most of the year, though a small shower will turn them into raging torrents for a few hours. There are several major streams which cross the plain from the western and southern highlands and they. too, are dry much of the year. The floor of the valley is broken in many places by stark mountain formations.

Vegetation is mostly confined to weeds and thin scrubby acacia bushes except along stream beds, where very fine trees may grow. Some sections of the plain are almost devoid of cover. However, last July, the Lodwar region had several showers of rain, and within a few days there was an astonishing amount of plants and flowers.

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* Columbia University, New York, N. Y. This paper, illustrated by motion pictures, was presented at the meeting of the Section on April 26, 1948.

There are parts of two large tribes living in this northwestern section of Kenya Colony: the Turkana, who are completely pastoral, and the Suk, many of whom combine agriculture with a pastoral life. There are now some 75,000 Turkana, wandering, with their livestock, over 25,000 square miles of the floor of the Great Rift Valley. They are a collection of similar ethnic groups which are, in turn, part of a still larger grouping of peoples all speaking mutually intelligible Nilo-Hamitic languages: the Donyiro and Topoth in the Anglo-Egyptian Sudan, the Dodoth, Jie, and Karamoja in eastern Uganda. The Kenya people refer to their language as Turkana and call themselves Turkana. Their closest ties are with the Jie, whom they consider blood relatives. According to legend, they were once a part of the Jie tribe living on the top of the Uganda Escarpment. A young bullock once strayed away and some of the young men who were hunting it came down to the upper Tarash River in what is now north-central Turkana. The country was empty and full of good berries and good grazing, so that there was a general movement into the new area. This migration probably took place less than 100 years ago. As a matter of fact, the Turkana have moved nearly 100 miles to the south since they were first contacted, and there is still a tendency to move to the southeast, which is resisted by the British administration.

The Turkana are excellent physical specimens. They appear to be an inch or so above the average European stature and are not inclined to body fat, though this latter may be due to food habits. To the eye, there seemed to be two generalized physical types, a short, heavily muscled man with many negroid facial traits and a taller type, lightly muscled and with fewer negroid traits. There are, of course, many variations of these types. However, without a thorough anthropometric study of the tribe, it would be difficult to determine just what part these generalized types play or even if they actually exist. Skin color ranges from coffee brown to black and the hair is universally negroid. The women average some four or five inches shorter than the men.

The health of the tribe is probably very good. Malaria does not play a major role in most of the area because of the extreme drouth. Conjunctivitis and other eye troubles are said to be fairly common. Venereal disease is unusual except around the stations, and Turkana girls who have contracted a venereal disease from some member of the native police patrol have been known to walk many miles into the station for treatment. The only physical malformation I noted commonly was ruptured navel. Possibly, the good health of the tribe is due to the fact that the life is hard and only the fittest survive.

Social Organization. The Turkana can be divided into sixteen major, named, territorial groups. In turn, there are twenty-eight groups of exogamous, nonlocalized, patrilineal sibs, most of which are represented in each of the major territorial groups. These patrilineal sibs are called *emacher* (brands) and each has a distinctive stock brand. Though these brands vary somewhat from group to group, I found that most of the stock brands collected around Lokitaung in the northern part of the district were readily recognized and named by Turkana around Karpeddo more than 200 miles south. The only functions of the patrilineal sib, so far as I could determine, were, the regulation of marriage, marking of stock, and payment of tax to the government.

The gentes are further divided into moieties. These are the *Ngimur* (stones) and *Ngirithai* (leopards). The son always belongs to the moiety opposite to that of which his father is a member. Strangely, in a system which alternates by generation, the members of *Ngimur* have somewhat more prestige than the *Ngirithai.* To the former is confined the right to wear black ostrich plumes, aluminum or iron ornaments, and giraffe's tail. The latter reserves the right to use white ostrich plumes and brass or copper ornament. Each moiety has its peculiar song subjects, though some ornament and songs are common to both sections. These moieties now seem to have little social or ceremonial function and may be on the wane. Girls are said to belong to their father's moiety until they marry, when they change to the husband's moiety.

Another set of social groupings is based on age. Males pass through four age groups: *Ngabanak,* (all pre-adolescent boys), *Esorok,* (post-adolescent boys), *Ngiliok,* (adults of warrior age), and *Ngathiku,* (aged). There are four similar age groups among women.

Puberty ceremonies are held for boys, and after the ceremony, which consists of the slaughter of a bullock by the candidate and the making of his first head-dress by his sponsor, the boy is a member of the *esorok* group. This is the only age group with a formal entrance ceremony. The Turkana do not circumcise. In addition to the four age groups through which each Turkana male must pass, the youth becomes a member of a permanent age set. These sets are created at about five-year intervals and are given such names as Zebras, White Camels, Full Rivers, etc. A man remains in his age set until death, and his set becomes senior as the older groups die off. The age groups and sets seem to play very little part in the life of the Turkana.

Domestic Life. The sparse browsing available for the livestock has forced a nomadic life on the people. The *awi* (village) is usually made up of an extended family—a man, his wives and children, and other assorted relatives. The *awi* may have 10 or as many as 40 people living in it. Except along the three or four streams which carry seasonal runoffs from the highlands, agriculture plays no part in the economic life of the people, and even in these places the crop is confined to a few small kaffir corn patches.

The whole subsistence basis of life is livestock: cattle, sheep and goats, camels, and donkeys. In spite of the poor grazing, cattle are found in all parts of the valley floor. The original wealth basis of the Turkana must have been built around cattle. Sheep and goats are well suited to the climate and terrain and are increasing so rapidly that the British administration is faced with the difficult task of reducing their density to prevent overgrazing. Both camels and donkeys are very well adapted to this country and one would have to go far to find better specimens of either.

All livestock is milked except donkeys, and I have heard that some Turkana milk the donkey. However, none of the animals can give much milk on the thorn bush diet. The people drink the milk fresh or soured and eat it clabbered. Some of the milk is churned in a wooden jug with skin ends to make *ghee,* a liquid butter which is eaten or used to grease the body.

An important addition to the milk diet is blood. The animal to be bled has a noose put around its neck and pulled fairly tight. This causes the superficial veins of the neck to swell. A vein is punctured with an arrow shot from a small crude bow held a few inches from the animal's neck. The arrow is unfeathered and has a metal blade set in the foreshaft. This is the only use the Turkana make of the bow and arrow. Stock bleeding is usually done by young men, but I have seen it done by boys and girls. After the vein is punctured, the blood is caught in a wooden bowl and stirred with a stick as it flows. The blood clots on the end of the stick and the remaining blood serum is either used raw and fresh or it is mixed with milk and drunk. As much as half a gallon of blood may be drawn off at one time from a cow or a camel, and the animal is not bled again for two or three months.

After the morning milking at the *awi*, the herds are sent out to graze separately in the charge of the young men, boys, and girls. By mid-morning, the herders have started the stock for the water holes along the river. In some areas, the Turkana are fortunate enough to have access to permanent water holes, but most of them must excavate wells in the dry sandy bottoms of the river beds. The job of scooping up the water and handing it up to the water trough falls on the young girls and women. Most of the water holes I saw in central Turkana were only four to six feet deep, but in the dry parts of southern Turkana these holes may go down twenty or more feet and four or five girls are needed to pass the wooden bowls of water up from the bottom.

Stock is watered every day if possible, but in very dry seasons it may be done every other day. Camels may even go three days between waterings.

The whole life of the people is centered around their livestock, but in spite of this they are very casual stockmen. No thought is given to breeding or care. Some Turkana are unnecessarily brutal to the animals.

In addition to the blood and milk diet, meat is eaten when an animal dies or when it is slaughtered for a ceremonial occasion. Such wild resources as are available are used. There are several types of berries and roots which are edible. From the few stores at the stations, the Turkana buy or trade for maize meal, tea and, most important of all, tobacco. The tobacco is a strong fibrous mixture which both men and women set great store by.

Some hunting and trapping is done, but game becomes more scarce by the year. The only animal present in any quantity today is Grant's gazelle.

Though the Turkana were not originally a fishing people and many of them will not eat fish, some of the groups living along Lake Rudolf have taken to fishing. At Namoruputh on the Ethiopian border, a fairly large permanent village, which depends largely on fishing and trade with the Merille, has been established. Fishing is a communal activity and is done by a drive through the shallow water, each man using a trap of net over a hemispherical wooden frame.

Political Organization. Until the British arrived, there were no chiefs or headmen. Each *awi* functioned under the leadership of the paternal head of the family. In any problem which arose affecting a section of the tribe, decisions were made by a group of the elders, and their prestige was such that the decision carried weight with the section. There was great difficulty in organizing anv reasonable part of the tribe for warfare, so this was confined to raids and counterraids by the younger men. For purposes of administration, the British have appointed chiefs and headmen in the territorial sections of the tribe, and the system seems to be working very well.

The Village. Away from the two stations, it is unusual for an *awi* to be in the same location for more than two or three months. Houses are little more than rough brush shelters. There is a low, dome-shaped bower which is covered with skins or with long grass if it is available. A few feet away, there will be a larger shelter (7 to 9 feet high), shaped like a large dome cut in half. Each wife has a similar set of these shelters, which she builds for herself. The smaller one is used by the women and children as a sleeping hut and the larger provides shelter from the sun and a place to work during the day.

The whole *awi* is surrounded by a thorn fence, inside of which are smaller fenced areas for the various sorts of livestock. The *awi* is built by the women and girls. Because of the dry air and frequent moves, the villages stay reasonably clean. They are not nearly so fly-infested as the semi-permanent villages of the Karamojong.

When the decision has been made to move to better browsing areas, all the personal effects and household equipment of the *awi* is loaded on the backs of donkeys and the move takes place. Camels are not used by the Turkana for transportation. A characteristic feature of the Turkana landscape is the large number of abandoned villages.

Dress and Ornament. Most of the emphasis in male costuming is placed on the head-dress. The hair on the back of the head is mixed with gray clay and molded into a bun. Built into the bun are several coils of wire which make plume sockets for ostrich plumes. Another smaller clay bun is built on the forepart of the head. Some men wear the curved handle of a calabash curving forward over the head from the rear bun. Others use a more spectacular arc-shaped rod made from thin strips of horn, on the end of which is a pom-pom of feathers. These head-dresses are most attractive, but delicate, and a man must use a wooden pillow for sleeping. During the day, the wooden pillow serves as a stool and is carried everywhere (see FIGURES 1 and 2 for illustration).

The ears are pierced for small aluminum rings and the septum of the nose is pierced so that a large leaf-shaped shield can be suspended over the mouth. One of the most striking ornaments is the labret of ivory or aluminum worn in the lower lip. Some of the ivory labrets are round and almost as large as a billiard ball. Brass or aluminum wire is wound around the upper arms and many brass or iron bracelets may be worn. Today, many men wear an "*americani*" cloth toga knotted on the right shoulder, but a few years ago men preferred a skin spine pad and a skin buttock cover. The genitals are not covered. All men wear leather sandals. Small boys do not bother with clothing of any sort.

From infancy, girls wear a small triangular leather apron trimmed with ostrich egg shell beads, and later a longer skin buttock cover is added. Except in size, these aprons do not change until the girl has reached the age of puberty, when she starts wearing a rectangular-shaped apron with the hair left on the outside. The back apron also has the hair left on, and it has heavy gussets sewed in to help the apron swing when the girl walks. This apron hangs as far down as the middle of the calf. In addition, a leather overall tied over the right shoulder and leaving the left breast bare is often worn.

The hair is shaved on both sides of the head, with a center roach left. The hair in this center piece is twisted into strings with red clay and fat. A woman wears many small earrings and as many necklaces of trade beads and seeds as she can come by. Two anklets made of the discarded ends of spears are worn on either leg. Both the lower and upper arms may be bound with aluminum or brass wire. Women also wear labrets, but never the large ivory balls used by the men.

Weapons used here are characteristic of most of the tribes to the north and west. The men carry one or two spears with leaf-shaped blades, a center shaft of wood, and an iron shoe. The spear may be seven to nine feet long, and, when carried in pairs, they are usually very well matched. The blade is covered with a leather guard. Every man carries a stick with a hockey-club end, and most men wear wrist knives (see FIGURE 3). This is a circular flat knife with one side opened so that it can be slipped on the wrist, which is protected by a leather guard; the edge of the blade also has a leather guard. The knife is worn on the right wrist.

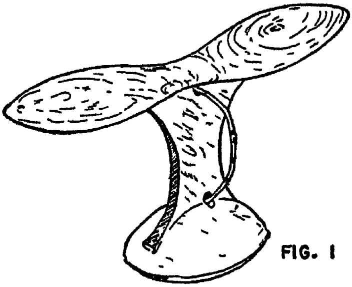

FIG. 1

HEIGHT 6 IN.

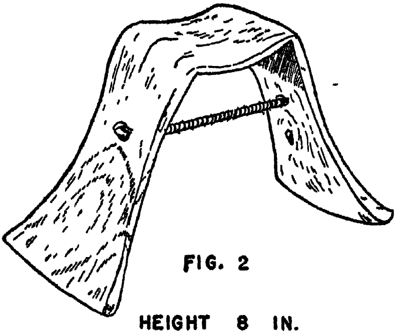

FIG. 2

HEIGHT 8 IN.

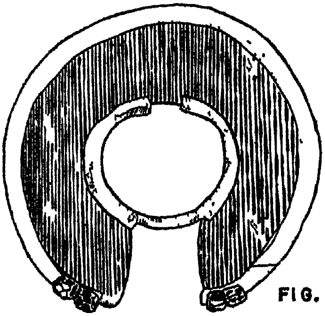

FIG. 3

DIA. 5 IN.

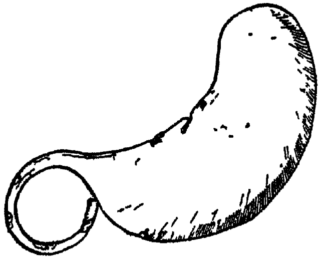

FIG. 4

LENGTH 4 IN

FIGURES 1-4.

Another murderous knife (FIGURE 4) is a large blade worn as a ring on the middle finger of the right hand. This knife does not have a leather guard. A narrow rectangular shield made of tough hide or of wicker was once carried by the Turkana, but these shields are becoming rather rare now.

Dancing. The favorite pastime is dancing, and any occasion which brings together several families may call for a dance. There are war dances, dances for the evening or day, hunting dances, and dances to bring good fortune. The Turkana dance very well and seem tireless. A dance which started one day at noon at Namoruputh was still going strong two days later. One of the most popular dances is the *Adonga,* in which the men sing about their cattle and jump up and down in a stiff-legged, rigid see-saw with the women, who dance facing the men, jumping on the off-beat. Women take part in nearly all of the dances, some of which are, in fact, rather erotic in character.

Conclusion. From both literary sources and native tradition, it appears that the Turkana are relatively recent migrants into the plains on the floor of the Great Rift Valley. They came as cattle-herding pastoralists and are being slowly reduced to sheep and goat herders by the aridity of the country.

Prestige which was once based on success in war and cattle raids and on the ownership of large herds has been terminated by the British administration, which has stopped the raiding, as well as by the poor grazing which will not support great herds. No new prestige substitute has been found; yet, the Turkana are a thriving people, well adjusted and happy. The population is increasing and the penetration southeastward still continues in the guise of labor on roads and on farms. Though the Turkanas seem to be one of the poorer tribes in Kenya, from a material standpoint, their material culture has penetrated all the neighboring tribes while they, themselves, have resisted influences from the outside.

The social organization, which is now so simple, shows traces in the decoration moieties, age sets, and age groups of once having been more elaborate and complicated. It is interesting to speculate on the reasons for the atrophy of some of these social elements—whether it is a result of the dislocation of a small, tightly organized tribe into a great empty area, or whether it is part of a process going on in all the kindred peoples of the Turkana. Only considerably more research among the tribes to the north and west can answer this question.

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ON THE CONCEPT OF CREOLE CULTURE IN PERU[1]

By HARRY TSCHOPIK, JR.*

The importance of understanding Latin America and Latin Americans is so obvious as to require no justification. During World War II, the need for inter-American solidarity was very real and immediate. The peace-time need for continued cooperation based on mutual understanding is no less urgent. "Any program of intelligent planning for human adjustment either for the present or for the future," wrote Gillin,[2] "must rest upon sound information concerning the physical make-up and cultural and social development of the population." It is in this field that anthropology is in the position to make a unique contribution. Because of his professional training and his first-hand knowledge of Latin American society and culture, the anthropologist is able to describe and classify these various ways of life in terms intelligible to the members of another culture. By detailing the content and orientation of the Latin American cultures, he provides a concrete basis on which statesmen, social planners, and business men may act, if they will. The proposed agenda for the forthcoming Second Inter-American Conference on Indian Life, moreover, make it abundantly evident that, throughout Latin America, those responsible for action programs—in education, public health, economics, and in many other fields—feel the need for more workable classificatory schemes descriptive of the various ways of life encountered in Latin American countries.

One of the most recent of the classificatory devices which has as its objective the delineation of a way of life in Spanish America is John Gillin's concept of "Creole" culture. It is the purpose of the present paper to examine the utility of this concept with special reference to a particular Latin American republic—Peru. First, however, it will be necessary to consider very briefly some problems which relate to Spanish America as a whole in order that the Peruvian situation may be seen in proper perspective.

I

In recent years, ethnological investigations and community studies in Latin America have indicated repeatedly that the cultures of the contemporary aborigines inhabiting that region formerly occupied by the pre-Columbian high civilizations of the New World—a region which extends roughly from the Bolivian plateau to the Valley of Mexico—may not be considered "Indian" in the sense that anthropologists are accustomed to speak of the "Indian cultures" of, say, the southwestern United States and of the Plains. In the latter areas, aged Indians may be found who are able to recall (usually through stories related to them by now-deceased grandparents) the tribal way of life as it existed before pronounced White contact. In Spanish America, however, after some four hundred years of European cultural domination during the Colonial era and afterward, the cultures of the contemporary aborigines have become so mixed that, in some cases, it is next to impossible to know what patterns are of native origin and what are not.[3]

This is not meant to say that, in the Latin American field, we are never justified in speaking of "Indian culture"; it simply means that the framing of "Indian culture" differs somewhat from current North American usage. In such a Latin American republic as Peru, for example, it is impossible to discover a single highland community whose culture has not been influenced in some measure by Spanish tradition. Nor is there a single living individual who is able to recollect the time when the way of life was wholly native. Under these circumstances, then, "Indian" employed in a cultural sense becomes a relative, not an absolute, term. In a very general way, we may define Indian culture as a way of life in which the content and orientation are predominantly indigenous.[4] For the sake of clarity, we will employ the term "Recent Indian" in the sense used by La Farge to differentiate between contemporary indigenous culture and that of the pre-Columbian aborigines.[5] By the same token, cultures in which the indigenous patterns have ceased to predominate (or in which they never figured to any appreciable extent) are "non-Indian."

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* Department of Anthropology, American Museum of Natural History, New York, N. Y. This paper is part of an address, illustrated by motion pictures and recorded music, which was delivered at the meeting of the Section on November 24, 1947.

Such arbitrary distinctions, it must be remembered, represent the judgments of the outsider, since the ways in which Peruvians classify themselves vary from one region to the next, and there is no general agreement as to what comprises Indian culture.[6] Insignia of culture are viewed as insignia of class, and what characterizes "Indian culture" and "Indian class" in one region of Peru often symbolizes "Mestizo culture" and "Mestizo class" in another. This is to say that culture patterns are taken to be symbolic of certain ways of life, and these in turn are rated in hierarchal structures which vary locally. No classificatory scheme which attempts to break down the entire population of Peru into its socio-cultural components by means of a *single set* of criteria will produce a picture which corresponds at every point to the diverse native formulations of culture and class.[7] When operating within a particular local framework, the judgment must, in the last analysis—and for purely pragmatic reasons—rest with the local people themselves, and we must reckon with their socio-cultural categories.

II

In view of the greater or lesser cultural admixture which characterizes the entire Spanish American scene (for, clearly, acculturation has not proceeded everywhere at a uniform pace), it was, perhaps, inevitable that some anthropologists should have come to interest themselves in the cultures of the Ladinos or Mestizos, peoples whose ways of life are, generally speaking, "non-Indian" and "non-Spanish" but represent highly complex integrations of these two, as well as other traditions. To date, no entirely satisfactory and generally accepted term has been devised to serve as an overall designation for the "non-Indian," "non-Spanish" cultures of Spanish America. Collectively, they have at times been called "Creole" (from Spanish, *criollo*), at other times merely "Latin American."[8] In an earlier paper, the writer labeled this way of life, "Mestizo."[9] Spanish American terms for this type of culture and its carriers are numerous and differ from one country to the next. To mention only the more familiar, "Mestizo" is employed in Mexico,[10] "Ladino" in parts of Central America,[11] and "Cholo" in Ecuador, Peru, Bolivia, and elsewhere.[12] Local usage, however, is invariably loose and inconsistent, and the problem of socio-cultural identification is further complicated in many areas by the existence of such descriptive class terms as *los correctos* ("the correct ones") and *los tontos* ("the ignorant ones") in some Mexican towns, *vecinos* ("citizens") and *forasteros* ("outsiders") in parts of Peru, and a variety of others.[13] In general, terms of this order tend to be employed in relatively homogeneous communities where class distinctions are a matter of economic standing, family tradition, education, and so forth, rather than basic cultural differences. Although restricted space prevents further discussion of the terminological problem at the present time, it will be necessary to return to this question in connection with our inquiry into the suitability of the term "Creole" as applied to certain aspects of contemporary Peruvian society and culture.

As descriptive data pertaining to the Ladino or Mestizo communities began to accumulate, it soon became apparent that, from Mexico to Peru, there were certain important underlying uniformities, both in regard to overt behavior patterns and to more basic attitudes and modes of thinking, which could not be explained in terms of any recent historical contacts. There are many patterns and situations in the cultures of Mitla and Tepoztlan in Mexico, for example, which are duplicated with a remarkable degree of exactitude in some Peruvian communities of the coast and highlands.[14] It is highly unlikely, however, that any resident of these Mexican towns has ever visited Peru, or that any citizen of Moche or Sicaya has traveled to Mexico. The obvious explanation, of course, is that the various Mestizo and Ladino communities of Spanish America exhibit certain clear-cut similarities because they share a common archaic Iberian heritage and because the stamp of Spanish Colonial culture is everywhere present.[15] It was the presence of these broad, basic resemblances, lending a certain apparent external unity to Spanish America, which led Gillin to formulate his concept of "Creole" or "Latin American" culture.[16] There is much to be said in favor of his proposition that "the Creole cultures of Spanish America . . . have a common general framework and a common tone which enables them to be spoken of collectively as *the* Creole culture and to be compared with *the* North American culture . . ."[17] To date, however, the formulation of "Creole" or "Latin American" culture has been couched in journalistic or impressionist terms, and meticulous analysis has been conspicuously absent. Although the hypothesis still remains to be demonstrated by means of systematic documentation, Davidson has recently made an attempt to define the content of "Latin American" culture in a highly tentative way.[18] While it appears that this type of culture is to be found in all the republics of Spanish America, there are said to be areal, regional, and local forms which are variable and may be distinguished from one another. Gillin has pointed out that, although important similarities exist throughout the area, the "Creole" cultures of Guatemala and Peru are clearly distinguishable, and that subconfigurations exist within each country.[19] Thus, he recognizes that, in Peru, the "Creole" culture of the highlands differs from that of the coast, while certain patterns differentiate the coastal towns of Moche and Cañete.[20] He also states that "Creole" or "Latin American" culture may be manifested in a variety of social situations within a given republic, and that "rural" and "urban" phases may be discerned.[21]

In the conclusions of his study of Moche, Gillin has recently made the plea that we recognize the contemporary "Creole" cultures of Spanish America as cultures in their own right, as new syntheses and reintegrations in new contexts of elements from diverse sources.[22] With special reference to the situation in Peru, he writes that "the Creole culture, which some day may justly be called Peruvian culture, is not a servile copying of either foreign or indigenous models, but a new and vigorous expression of national life."[23] While this "emerging Creole culture" is still in the process of consolidation, "the Peruvian Creole culture is a new synthesis."[24] In brief, Gillin sees contemporary Peruvian culture as divisible into two major types: Recent Indian (which, following Kubler, he designates "Republican Native") and Creole.[25]

III

Let us now examine this position point

by point, returning first to the vexing problem of terminology. As we have seen, Gillin has designated the "non-Indian", "non-Spanish" culture of Spanish America "Creole," and has also applied this term to a particular culture which he postulates as emergent in Peru. He objects to the use of the word "Mestizo" in a cultural sense because it carries with it the implication of race mixture.[26] "At present," he writes, "some pure Indians on the one hand and some pure whites on the other hand, *as well as most Mestizos,* participate in the development and performance of the Creole culture . . ."[27] Those who participate in the Recent Indian cultures of Peru, however, are either pure Indian racially or at least have a proportionately large amount of Indian blood.[28] While it is true, of course, that there appears to be no inherent relationship between race and culture, it is equally true that genetic hybridization in Peru has everywhere accompanied acculturation, and it is an undeniable fact that the majority of the carriers of "Creole" culture are, genetically, Mestizos.

As a cultural term, "Creole" is scarcely more suitable than "Mestizo," since biological connotations are equally implicit—*i.e.,* the purity of strain, rather than race mixture, is stressed. With regard to the use of the word in nineteenth-century Peru, Tschudi wrote: "The designation Creole properly belongs to all the natives of America born of parents who have emigrated from the Old World, be those parents Europeans or Africans. There are, therefore, white as well as black Creoles."[29] That the biological emphasis was paramount is unquestionable: "The Spaniards apply the term *criollo* not merely to the human race, but also to animals propagated in the colonies, but of pure European blood . . .".[30] Although, at the present time, the word is employed in a variety of contexts, Romero uses *criollo* to mean *"descendiente directo del español."*[31] In addition to its original racial meaning, however, the term has been extended by usage, and has taken on certain cultural implications. Often, it designates what is native as opposed to what is foreign. The *"vals criollo"* is a native waltz (in the Spanish tradition) in contradistinction to the American or Bavarian waltz, while *"criollo* food" refers to dishes prepared in the Lima manner rather than to foreign food or to other native cookery. The term may even be employed in a loose, nationalistic sense; golf matches in Arequipa between British and American club members on the one hand and Peruvian members on the other were often framed as *"gringos" versus "criollos."* In Lima, the word has the added significance of one born in the capital as opposed to the *"serranos,"* or highlanders.

The question, however, of whether "Creole" or some other term should be employed to designate the "non-Indian," "mixed" cultures of Peru would be idle and inconsequential were it not for the fact that *"criollo"* (or Creole), as a cultural term, should be reserved more properly for a way of life which is vastly different from that of the Mestizos, or of the Indians.[32] The true *criollo* culture of Peru, unfortunately, has received no systematic attention in scientific literature; it has attracted neither the anthropologist nor the sociologist. Yet descriptions of it are to be found in memoirs, diaries, and journals, and especially in Peruvian novels.[33] Strictly speaking, *"criollo"*, employed in a cultural sense, designates a post-Colonial Spanish way of life which, fundamentally, is rooted in neither archaic Iberian nor in indigenous Peruvian culture. It derives rather from the late eighteenth- and nineteenth-century Spanish culture, and is tinged with strong French influences. Although no summary of content is attempted here, the way of life of the Lima *criollos,* as described by Tschudi, reveals virtually nothing of "Indian" tradition.[34] As its carriers were true *criollos* racially, so the culture was *criollo* in a very real sense, modeled in the image of

Spain. This Spanish derived and oriented *criollo* culture is preserved today as the class culture of the old-guard aristocrats of such cities as Lima and Arequipa, and, to an even greater extent, of Trujillo, Piura, and Tacna. Since it is today exclusively a class culture, and is nowhere shared by all the members of any community, this way of life tends to be designated by class terms. In Lima and Arequipa, it is usually called *"alta sociedad"* ("high society"), while in Cuzco it is known as *"jailé"* (from English "high life"). Gradually, owing to increasingly intensive contacts with other contemporary cultural centers, especially London, Paris, and New York, and to the progressive breakdown of regionalism in Peru, *criollo* culture is tending to lose its marked Iberian bias and to merge more and more with modern Western civilization.

To sum up, then, it appears singularly inappropriate to employ a term which denotes purity of strain to designate a culture of mixed Indian and Spanish tradition, especially when another term, in which the concept of mixture is implicit, is ready at hand. The mixed culture, characteristic of most Peruvian Mestizos, will hereafter be called "Mestizo" culture; "Creole" (or *"criollo"*) will designate a variant of post-Colonial Spanish culture whose carriers are, in the main, the Peruvian-born descendants of Spaniards.

Let us now consider the content of "Creole" culture as defined by Gillin and as outlined tentatively by him in the conclusions of his Moche report. In all honesty, it must be stated that he attempted no formal analysis, and that his remarks are intended merely "to suggest that Creole culture is a synthesis of elements drawn from various sources and that the Spanish stamp gives to this general mode of life a certain external uniformity . . ."[35]

Yet an examination of the patterns which are taken to be the diagnostics of "Creole" culture reveals at once that these are *all*—with the exception of such aspects of the intellectual content as ideas borrowed from Marxism, the Enlightenment, and from the French and American Revolutions—of Spanish origin.[36] They include such traits as Iberian Catholicism, concepts of "personal honor" and of Roman law, the plaza in town planning, the use of broad-brimmed hats, ceremonial kinship, and such features of domestic architecture as the patio and the barred window, to mention but a few. But although Gillin writes that ". . . almost everywhere the Creole culture has been strongly influenced by aboriginal elements," no aboriginal patterns whatever figure in the cultural content as formulated.[37] The exclusive emphasis on the Spanish patterns tends to obscure the fact that what really characterizes this way of life is the fusion and mixture of Spanish culture with Indian tradition. The very omission of the Indian elements suggests what we actually find to be the case, namely, that the indigenous components of Mestizo culture vary from one region of Peru to another, that there appear to be no indigenous common denominators or, at best, very few.

Let us compare very briefly the indigenous components of three Peruvian Mestizo cultures: Yucay, Sicaya, and Moche.[38] (For a general map of Peru, see FIGURE 1.) What follows is not intended to constitute a definitive analysis, but rather to suggest that the indigenous content of Mestizo culture in one region is apt to be almost totally dissimilar to that of another. In respect to the three communities considered here, the Indian-derived patterns listed below which are characteristic of any one are not, to the best of the writer's knowledge, shared by the other two.

Some indigenous components of Yucay Mestizo culture may be listed as follows: agriculture ritual involving libations in the fields before plowing; use of the foot plow in farming; barter in trade; jerked llama meat a staple of diet; sandals habit-

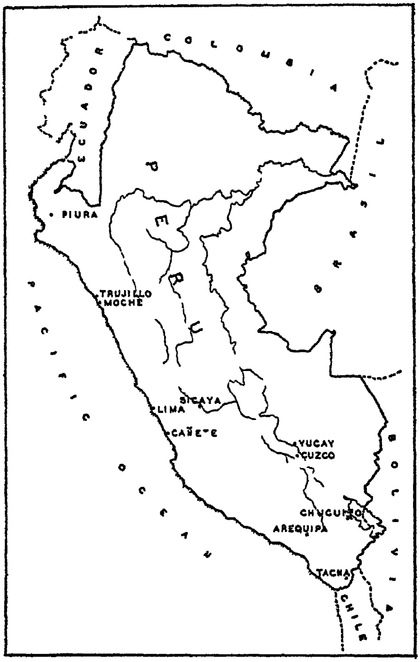

Figure 1.

ually worn by both sexes; wrap-around skirt for toddlers; knitted caps for children, and worn in bed by adults; end flutes and pan-pipes important musical instruments; Indian-type dance (*cacharpari*) at Carnival; beliefs that certain diseases are caused by evil springs, the earth, the rainbow, and by souls of the dead; belief in a type of witch who steals human fat; belief in the *Ccoa,* a supernatural cat, the sponsor of sorcerers[39]; divination with coca leaves; belief in a flying head demon.[40]

The following Indian-derived patterns are components of the Mestizo culture of Sicaya: animal increase rites; labor exchange, *uyay,* in house building; Indian-type distaff in spinning; moiety division of the town; *palomay,* moral advice given to newlyweds by an aged relative or respected citizen; *palpa,* presents of livestock, building timbers, or cash given to newlyweds; newborn infant's umbilical cord cut with a potsherd; joking and games at funerals; funeral feast for relatives and friends; food, coca, and alcohol offered to the dead; coca chewed ceremonially; separation of the sexes on ritual occasions; belief in the *amaru,* a thieving mythical serpent-monster.

The Mestizo culture of Moche has incorporated such indigenous patterns as the use of meandering irrigation ditches; Indian fishing techniques, and the importance of fish, crabs, and shrimp as foods; the eating of lizards and iguanas; drinking gourds; balsas; wattle-and-daub houses; sleeping mats; the practice of going barefoot; rite occasioned by the first cutting of a child's fingernails; the use of white corn meal for purification; the separation of "white" and "black" magic.[41]

Certain indigenous patterns, on the other hand, are shared by two of the three communities here considered, but are absent in the third. Witchcraft beliefs and the use of alcoholic *chicha,* for example, are characteristic of Moche and Yucay, but not of Sicaya. In both Sicaya and Moche, guinea pigs are employed in the diagnosis of disease, but this is not true of Yucay. Again, while guinea pigs are raised for food in Yucay and Sicaya, they are not in Moche. While the loss of certain Indian-derived patterns in a particular Mestizo community may be postulated to explain apparent differences in indigenous content when two Mestizo cultures are compared, such an explanation, to be acceptable, must be documented with proof, and such proof is usually exceedingly difficult to obtain. It is more probable, the writer believes, that such differences are to be attributed in the main to varying factors in the acculturation processes during the Colonial era, and to basic differences in the contents of the several indigenous cultures at the time of the Conquest.

Although the Spanish Colonial ingredients of "Creole" culture do lend a certain superficial uniformity to the component local cultures, closer inspection reveals that these are by no means identical wherever they occur. The patterns of ceremonial kinship, for example, differ greatly in degree of elaboration in Moche and in Yucay, and the Spanish-derived features of political organization which characterize Yucay are not identical with those of Sicaya. Many of the patterns, moreover, which are taken as diagnostics of "Creole" culture, are shared by groups who consider themselves (and are so judged by outside observers) to be Indians. Ceremonial kinship, the plaza in town planning, and elements of Roman Catholic ritual, to cite but a few examples, are common to Recent Indian and Mestizo cultures alike.

In addition to differences in content, the various Mestizo cultures of Peru are further differentiated in respect to general orientation. What appears to make the basic difference in orientation—and also, to a not inconsiderable extent, in content—is the place which a given Mestizo culture occupies within the framework of a particular local hierarchal structure. Thus, for example, in the village of Chucuito where Aymara Indian peasants comprise the bulk of the population, the Mestizos form a small, "aristocratic" class. As such, they abstain from all manual labor, practice no agriculture, engage in no handicrafts, and affect the manners and customs of the *criollos* of southern Peru.[42] Their way of life and general orientation is vastly different from that of the fishermen of Moche or the farmers of Sicaya. Thus, Mestizo culture might quite properly be termed "aristocratic" in caste-structured Chucuito, "rural" in classless Moche, and "middle class" in class-structured Arequipa.

It would appear, therefore, in the light of the evidence, that we are not justified in speaking of "*the* Peruvian Creole culture"; instead, we must recognize a series of local "Creole" or Mestizo cultures. Save for some patterns which derive from a common Spanish heritage, these local cultures are characterized merely by being neither predominantly indigenous nor predominantly Iberian Spanish. After the shared Spanish-derived patterns have been factored out, a miscellaneous hodgepodge of indigenous custom remains which is different in each region of Peru. It is true, of course, that each local "Creole" culture has achieved *an* integration, but this is not *the one and only* integration. We can subscribe to the proposition that, some day, we shall be able to speak justly of "Peruvian culture," but that day has not yet arrived.[43] *"Peruanidad,"* a word coined recently in Peru to describe a hypothetical national cultural unity, is not to be referred to any particular way of life participated in by all Peruvians. The word merely reflects the nationalistic political trend of the times; it designates the total cultural heritage of Peru, but has no specific locus in time or space.

Neither does it seem justifiable to speak of "Creole" culture as "emergent."[44] Each and every culture, of course, is a product of its total history, and since each is constantly in a state of change (however great or small), any culture, at a given point in its development, might properly be termed an "emergent" culture. The use of this word, however, carries with it an implication of recency, whereas it is evident that the processes of reintegration and synthesis were advanced in Spanish Colonial times.[45] "Creole" culture is not a new synthesis; rather, it is a reflection of what survives of Colonial Spain. What is happening in Peru today is that, owing to improved transportation, communication, and educational facilities, local cultures have recently begun to lose their distinctiveness. In all probability, this leveling process will eventually produce a national culture; but national culture in Peru is far from being an accomplished fact.

IV

Our chief concern in the foregoing discussion has been with certain problems of classification, and classifications, it must be remembered, are artificial constructs, not "objective realities." It may not be argued, therefore, that some are "true" while others are "false." The merit of a particular classification, so long as it embraces the "facts" without violence and distortion, may be judged only in regard to the degree to which it is useful or not useful in dealing with a given problem or set of problems. It is in this sense that we must evaluate Gillin's concept of "Creole culture."

The writer's principal objection to this term is that it is made to embrace two concepts of very different orders: (1) a "culture area"; and (2) a specific culture. The patterns which have been selected as diagnostics of this way of life are, with one exception, derived exclusively from Spanish culture. On the level of "culture area," the concept of *Creole* culture is most useful in calling attention to the fact that the various Mestizo peoples of Spanish America share some patterns which serve to differentiate their way of life from those of, say, North America or China. The choice of the word "Creole" is, nevertheless, unfornate for reasons stated above, and it would be preferable, perhaps, to substitute "Modern Latin American" or "Recent Spanish American," or some such term, as a label for "Creole" culture considered as a "culture area."

But while *Creole culture* has been used in a general sense to designate a *type of culture* (Mestizo) found throughout Spanish America, *the Peruvian Creole culture,* defined in terms of the same set of criteria, has been employed in a specific sense to designate a *particular culture* in Peru. The recognition of local, regional, and class variations merely serves to indicate that, used in the latter sense, the concept is too inclusive and all-embracing to contribute materially to a better understanding of the nature of Peruvian society and culture. Brew has pointed out the danger, in classification, of "overemphasizing single criteria or selected groups of criteria which, although of value in studying those particular characters, cannot fail to give a warped picture of the whole."[46] What is delimited by the diagnostics of Peruvian Creole culture is not only virtually pan-Peruvian; it is pan-Spanish American. The concept as formulated is too general to distinguish, for example, that which is peculiarly Peruvian from that which is Ecuadorian, nor is it wholly adequate to differentiate what is essentially Mestizo from what is Recent Indian. The mere sharing of some Spanish-derived patterns seems to constitute insufficient grounds for designating such very different Peruvian Mestizo cultures collectively as *a culture;* the external uniformity imposed by selection is illusory and misleading. "Creole culture," therefore, may be employed as a label for a type of culture or it may designate a specific cultural configuration and set of patterns. It cannot, however, be made to serve both purposes.

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NOTES

[1] This paper was benefited by the helpful suggestions and criticism of Drs. Wendell C Bennett, James A. Ford, and Sr. Gabriel Escobar.

[2] **Gillin, J.** (1942): 217.

[3] For excellent historical summaries of acculturation phases in two regions of Spanish America, see: **La Farge, O.** (1940): 281-291; **Kubler, G.** (1946): 331-410.

[4] For brief descriptions of contemporary Peruvian cultures which are predominantly indigenous in character, see: **Mishkin, B.** (1946): 411-470; **Tschopik, H., Jr.** (1946): 501-573.

[5] **La Farge, O.** (1947): xv. Care should be taken not to confuse the term "Recent Indian" with "El Nuevo Indio", a concept of J. Uriel García which, in a cultural sense, means "Mestizo" and not "Indian." (**García, J. Uriel,** 1930.)

[6] The definition of "Indian" in Peru, as elsewhere in Latin America, is primarily a problem of culture rather than of race. See: **Tschopik, H., Jr.** (1947): 11-14; **Tax, S.** (1942): 43-48; **Steward, J. H.** (1945): 293-294; **Gámio, M.** (1945): 410-415.

[7] The writer recently experienced great difficulty in attempting to apply uniform cultural criteria, for the purpose of identifying social groups, to a relatively small region of the Peruvian highlands. (**Tschopik, H., Jr.** (1947): 11-15.)

[8] **Gillin, J.** (1947a): 151; (1947b): 224.

[9] **Tschopik, H., Jr.** (1947): 14. Saenz has also employed "Mestizo" in a cultural, as well as a racial, sense with reference to the Peruvian situation. (**Saenz, M.** (1933): 261-278.)

[10] **Redfield, R.** (1941): 25, 74-76; **Beals, R. L.** (1946): 210-213.

[11] **Wisdom, C.** (1940): 222-225; **Gillin, J.** (1945): 1-14.

[12] **Parsons, E. C.** (1945): 1; **Tschopik, H., Jr.** (1947): 11, *n.* 6. The term "Cholo" is employed in a racial sense (to mean "mixed-blood") in Bolivia, where it is highly probable that the word also designates a type of culture as well. (**La Barre, W.** (1948): 45.)

[13] **Redfield, R.** (1930): 68; **Tschopik, H., Jr.** (1947): 38-39. For a discussion of the usage of such terms in four Yucatan communities, see: **Redfield, R.** (1941): 58-85.

[14] **Parsons, E. C.** (1936); **Redfield, R.** (1930); **Gillin, J.** (1947a); **Tschopik, H., Jr.** (1947).

[15] **Gillin, J.** (1947b): 244-245.

[16] For concise summaries of this concept, see: **Gillin, J.** (1947a): 151-160; (1947b): 243-248.

[17] **Gillin, J.** (1947a): 151.

[18] **Davidson, W.** (1947).

[19] **Gillin, J.** (1947a): 152.

[20] Idem.

[21] **Gillin, J.** (1947b): 246.

[22] **Gillin, J.** (1947a): 151-152.

[23] Ibid.: 153.

[24] Idem.

[25] Ibid.: 152; **Kubler, G.** (1946).

[26] **Gillin, J.** (1947b): 244.

[27] **Gillin, J.** (1947a): 151. Italics mine.

[28] Saenz cites some cases where individuals who are undoubtedly Mestizos genetically have "reverted" to a Recent Indian way of life and consider themselves to be Indians; such cases, however, must be regarded as most exceptional. (**Saenz, M.** (1933): 272.)

[29] **Tschudi, J. J. v.** (1847): 80. Similarly, in Louisiana under French domination, "Creole" designated those born in colonial territory of French parentage; the notion that "Creole" here implied a mixture of Negro and White is a popular misconception.

[30] Idem, footnote.

[31] **Romero, E.** (1928): 178.

[32] Gámio, too, finds it necessary to distinguish between "Mestizo" and *"criollo"* in respect to the Mexican situation. (**Gámio, M.** (1945): 407.)

[33] The memoirs of Flora Tristán describe in detail the life of the Arequipa *criollos* during the early nineteenth century. (**Tristán, F.**, 1946.) The late nineteenth-century *criollo* culture of Cuzco is abundantly documented in the novels and other writings of Clorinda Matto de Turner (1854-1909).

[34] **Tschudi, J. J. v.** (1847): 65-74, 89-110. For further details of Lima criollo culture, see: **Díez Canseco, J. L.** (1934).

[35] **Gillin, J.** (1947a): 152.

[36] Ibid.: 151-152.

[37] **Gillin, J.** (1948): 33.

[38] The data pertaining to Yucay were kindly furnished by Sr. Gabriel Escobar, a Peruvian citizen who resided in that town for a number of years. Sicaya was studied by the writer, Sr. Escobar, and Sr. Jorge C. Muelle on behalf of the Institute of Social Anthropology of the Smithsonian Institution and the Instituto de Estudios Etnológicos of Lima. Gillin's Moche report has furnished the data on that coastal community. (**Gillin, J.**, 1947a.)

[39] *cf.* **Mishkin, B.** (1946): 463-464.

[40] *cf.* **Tschopik, H., Jr.** (1946): 560.

[41] *cf.* **Tschopik, H., Jr.** (1946): 564-566.

[42] Chucuito was studied by the writer during 1940-1942 on behalf of the Division of Anthropology and the Peabody Museum of Harvard University.

[43] **Gillin, J.** (1947a): 153.

[44] Idem.

[45] **Kubler, G.** (1946): 345-353.

[46] **Brew, J. O.** (1946): 47.

SECTION OF PHYSICS AND CHEMISTRY

THE RATE OF DISSOLUTION OF METALS IN AQUEOUS REAGENTS

By CECIL V. KING*

THE DIFFUSION LAYER THEORY. In 1897, Noyes and Whitney published measurements of the rate of solution of benzoic acid and lead chloride in water.[1] They used cylinders of these materials, 8 x 2 cm. cast on glass rods, as cores; a cylinder was mounted in a flask containing water and the whole was rotated at about 60 r.p.m. To explain the results, they suggested that an extremely thin layer of saturated solution is formed at the solid surface and that the observed rate is determined by diffusion away from this layer.

In 1904 Nernst presented a clearer picture of this "diffusion theory."[2] He suggested that, since in many cases there is a very high difference in chemical potential at the interface, it is reasonable to expect an extremely fast chemical reaction. The observed rate would then depend upon the geometric relations of the apparatus used, the intensity of the convection currents set up, and the diffusion coefficient of the reagent. The various factors could be expressed in the equation

$$dx/dt = D\,A\,C/\delta \qquad (1)$$

where D is the diffusion coefficient, A the area of solid, C the concentration difference at the surface and in the solution, and δ the thickness of the layer in which a concentration gradient exists.

The theory was elaborated somewhat by Brunner,[3] who supported it with new experiments using benzoic acid; magnesia and magnesium metal dissolving in acids; and electrolytic experiments in which concentration polarization is found. Brunner realized that the concentration gradient might not be entirely linear and that the outer edge of the diffusion layer might not be sharply defined, and considered δ as an "effective" thickness which makes EQUATION 1 valid.

Five criteria are now generally accepted as indicative of diffusion-controlled rates[4]:

(1) Different solids dissolve at nearly the same rate in the same reagent, under the same conditions.

(2) The speed of stirring has a large effect, the exact nature of which depends on the type of stirring used.

(3) The dissolution rate is approximately inversely proportional to the viscosity, with other physical and chemical factors constant.

(4) The dissolution rates with a series of reagents are proportional to a fractional power (ca. 0.75) of the diffusion coefficients.

(5) The temperature coefficients are lower than for homogeneous reactions, usually 1.1 to 1.5 per 10° at room temperature.

NATURE OF THE DIFFUSION LAYER. The "film" or "layer" theory has found wide application in interpreting heterogeneous rates, as in the dissolution of metals and other solids, the transfer of heat to liquids or gases, the evaporation of liquids or solids, the absorption of gases or vapors, and in reactions at liquid-liquid interfaces. In most cases, the stirring or flow rates are comparatively low and it is often evident that concentration gradients extend much further from the interface than the theory implies. In this case, both convection and diffusion contribute to transport in an intermediate layer, and this has been called "eddy diffusion."

When rotating metal cylinders are used, the rate constant k becomes linear with peripheral speed above ca. 5,000 cm./min. (FIGURE 1). Under such conditions it is probable that "eddy diffusion" is confined to a layer much thinner than the "true diffusion" layer. Only the velocity component of fluid motion perpendicular to the solid surface is important in determining the relative effects of convection and diffusion on the rate.[5] It has been shown that turbulent flow near a pipe wall is essentially laminar because the perpendicular component (v) becomes very small.[6] It has been suggested[7] that the relation between dissolution rates and diffusion coefficients can be explained by an exponential increase of the turbulence component v with distance in the proper range (distance $d = 1$ to 50 microns).

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* Washington Square College of Arts and Science, New York University, New York, N. Y. This paper, illustrated by lantern slides, was presented at the meeting of the Section on April 6, 1948.

[1] Noyes, A. A., & W. R. Whitney. Z. phys. Chem. 23: 689. 1897.

[2] Nernst, W. Ibid. 47: 52 1904.

[3] Brunner, E. Ibid. 47: 56. 1904.

[4] King, C. V., & M. M. Braverman. J. Am. Chem. Soc. 54: 1744. 1932.

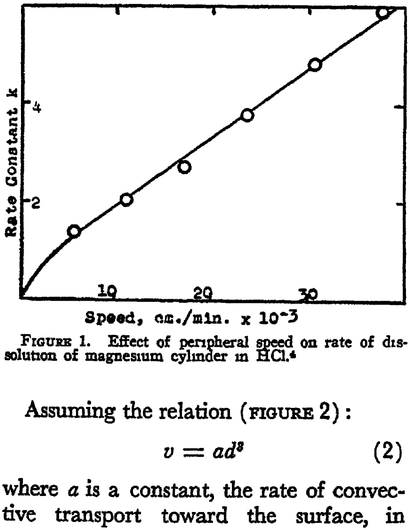

FIGURE 1. Effect of peripheral speed on rate of dissolution of magnesium cylinder in HCl.[4]

Assuming the relation (FIGURE 2):

$$v = ad^3 \qquad (2)$$

where a is a constant, the rate of convective transport toward the surface, in equivalents n of reagent per unit time is given by

$$dn/dt = ad^3\,AC \qquad (3)$$

where A is the area of solid, c the reagent concentration. Fick's law for diffusion through a linear concentration gradient is

$$dn/dt = D\,A\,C/\delta \qquad (4)$$

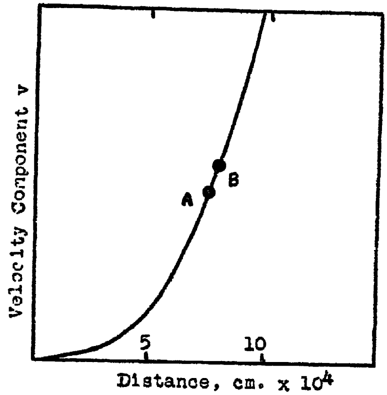

FIGURE 2. Suggested relation between perpendicular component of turbulent flow and distance.

where D is the diffusion coefficient. If it is assumed that eddy diffusion is found essentially in only a thin layer between points A and B of FIGURE 2, *i.e.*, that transport is sensibly only by diffusion to the left of A and that the solution is homogeneous to the right of B, EQUATIONS 3 and 4 may be combined by setting $d = \delta$ at an intermediate point. Then,

$$a\delta^3 = D/\delta \quad \text{and} \quad \delta = D^{1/4}/a^{1/4} \qquad (5)$$

Combining with the Nernst relation $k = D/\delta$, where k is the observed rate constant,

$$k = \alpha D^{3/4} \qquad (6)$$

where α is a constant. It has been shown that such a relation is obeyed experimentally and may be used to compare heat transfer rates with metal dissolution rates.[8, 9]

DEPOLARIZERS. Magnesium dissolves freely in dilute acids at a rate which is presumably controlled by diffusion of acid to the surface.[8] With potassium nitrate or certain other oxidizing agents present as depolarizers, zinc, cadmium and iron dissolve, under proper conditions, at practically the same rate as magnesium.[4, 10, 11] With excess acid, the rate is controlled by diffusion of the depolarizer; with excess of the latter, by diffusion of the acid.

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[5] **Van Name, R. G., & D. U. Hill.** Am. J. Sci. (4) 36: 543. 1913; 42: 307. 1916.

[6] **Fage, A., & H. C. H. Townend.** Proc. Roy. Soc. London A135: 656. 1932.

[7] **King, C. V.** Report of University Conference on Corrosion and Metal Protection, Chicago (June, 1947). To be published.

[8] **King, C. V., & W. H. Cathcart.** J. Am. Chem. Soc. 59: 63. 1937.

[9] **King, C. V., & P. L. Howard.** Ind. Eng. Chem. 29: 75. 1937.

[10] **King, C. V.** J. Am. Chem. Soc. 57: 828. 1935

[11] **King, C. V., & M. B. Abramson.** Ibid. 61: 2290. 1939.

Evidently, the function of a depolarizer is to avoid the effect of hydrogen overvoltage on the metal, and it may do this by making hydrogen evolution unnecessary. With zinc and nitrate ion in acid solution as electrode reagents, the following equations might apply:

$$\begin{gathered} Zn - 2\varepsilon \rightarrow Zn^{++} \\ NO_3^- + 10H^+ + 8\varepsilon \rightarrow NH_4^+ + 3H_2O \end{gathered} \tag{7}$$

Adopting the older theory that hydrogen overvoltage is due to the slow combination of H-atoms to form H_2–molecules, we might write:

$$\begin{gathered} Zn + 2H^+ \rightarrow Zn^{++} + 2H \\ NO_3^- + 2H^+ + 8H \rightarrow NH_4^+ + 3H_2O \end{gathered} \tag{8}$$

Depolarization would be explained by the rapid removal of H-atoms from the surface by nitrate.

Newer theory[12] postulates two energy barriers in the electrical double layer at the metal surface, with overvoltage caused by the slow passage of protons across one of the barriers. A depolarizer might conceivably function by lowering the energy barrier or by sidestepping passage of protons over the barrier while being reduced itself.

The lowest reduction potential at which an oxidation-reduction system could be expected to bring about reaction would be near the standard reversible potential of the metal itself, namely —0.76 volt in the case of zinc. Set up with a reversible electrode, any system of higher potential would cause zinc to dissolve, but in the case we are considering, if the initial attack on the metal is by acid, a somewhat higher potential may be necessary. The experiments described below were undertaken to find the potential which an oxidizing agent must possess to act as a depolarizer with zinc.

NEW EXPERIMENTS. Zinc cylinders, 2.54 cm. long and about 1.9 cm. in diameter, were rotated at 3100 to 3150 r.p.m. in the solutions mentioned below, at 25° ± 1° C. Nitrogen was bubbled through the solutions to remove oxygen and to prevent reoxidation of reduced depolarizer. The C.P. zinc used dissolves at an appreciable rate in dilute acid alone and this rate increases with time. Blanks were run but could not be used consistently as corrections. Some depolarizers appear to eliminate or minimize this spontaneous rate except when very dilute.

Potassium nitrate. It has been shown previously[18] that, with excess potassium nitrate and dilute acetic acid buffer, first-order rate constants are obtained over more than half the reaction assuming reduction to nitrite. New work indicates that, with excess hydrochloric acid, nitrate is reduced to ammonium ion. The rate is not first-order under the conditions used (FIGURE 3), but the total amount dissolved agrees well, as shown in TABLE 1. In the acid alone, 10-minute blanks gave 1.8 and 1.3 mg. losses, and there is a similar weight loss after all nitrate is reduced.

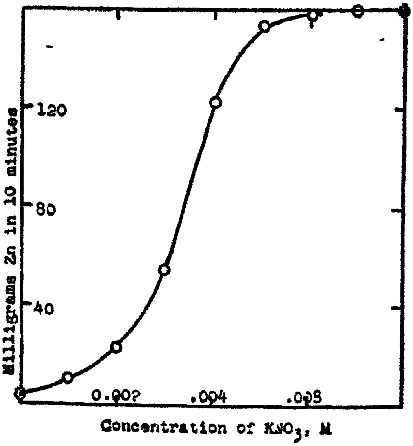

FIGURE 3. Rate of dissolution of zinc cylinder 2.54 x 1.92 cm., 3150 r.p.m., 1000 ml. 0.0182 M HCl + KNO_3, 25°.

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[12] Eyring, H., S. Glasstone, & K. J. Laidler. J. Chem. Phys. 7: 1053. 1939; Kimball, G. E., S. Glasstone, & A. Glassner. Ibid. 9: 91. 1941.

[18] King, C. V., & M. Schack. J. Am. Chem. Soc. 57: 1212. 1935.

The zinc was weighed and polished at intervals given. Values of k in TABLE 1 were calculated as if the rate were first order, using the equation

$$k = \frac{2.3\,V}{A\,t} \log \frac{a}{a\text{-}x} \qquad (9)$$

where V is the volume of solution, a the total theoretical amount of zinc dissolved.

TABLE 1

ZN CYLINDER IN 100 ML. 0.091 M HCL, 0 004 M KNO_3, 25°, 3100 R.P.M.

Time, min	Zn dissolved, mg.	k
5	43.1	0.65
10	35.9	.59
10	14.8	.57
20	10.2	
10	3.8	—total 107.8
10	1.8	theory 104 6

The irreversible potential of the nitrate-acid system is sufficiently high to oxidize zinc at a diffusion-controlled rate at all feasible values of pH.

Quinone. The quinone-hydroquinone system was chosen as one of known reversible potential which varies with pH. TABLE 2 shows that, with hydrochloric acid and excess quinone, the rate is first-order with respect to HCl although the potential decreases considerably as acid disappears and hydroquinone is formed. With excess acid, the rate is nearly first-order with respect to quinone; k increases somewhat with time, probably because a little hydrogen is evolved. The total metal dissolved in 55 minutes was actually 214 mg. The higher rate in excess quinone is typical of the higher diffusion coefficient of the hydrochloric acid.

TABLE 2

THE RATE IN ACID AND QUINONE SOLUTIONS

Time, min.	x, mg.	k
200 ml. 0.0182 M HCl, 0.04 M Quinone		
3	50	2.46
3	31.5	2.62
4	20.7	2.62
5	9.2	2.46
∞	111.4 (theory)	
200 ml. 0.091 M HCl, 0.015 M Quinone		
4	55.9	0.99
6	50.8	1.05
6	33.7	1.04
6	23.0	1.08
∞	196 (theory)	

TABLE 3 shows the effect of adding hydroquinone (HQ) and of using an acetic acid buffer. Values of k are for the first 4 to 11 minutes, and the approximate potential E is for half-reduction of the quinone. The smaller rate in the buffers is partly (4%) due to the higher viscosity, may be partly due to the lower potential.

Methylene blue. The potential of the methylene blue-methylene white system has been studied by Clark, Cohen, and Gibbs.[14] Over the range of pH in which

TABLE 3

THE RATE IN STRONG AND WEAK ACIDS

Acid	Quinone	H Q	k	E, volt.
0.091 M HCl	0.01M	——	0.95	0.64
	.01	.01M	.95	.62
	.01	.02	.97	.62
	.01	.03	.96	.61
0.108 M HAc,	.01	——	.86	.40
0.10 M NaAc	.01	.01	.85	.39
	.01	.02	.84	.38
	.01	.03	.82	.37

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[14] Clark, W. M., B. Cohen, & H. D. Gibbs. U. S. Public Health Service, Hyg. Lab. Bull. 151: 174. 1928.

we are interested, the potential at half-reduction is given approximately by the equation

$$E = 0.53 - 0.09 \log 1/C_{\mathrm{H}^+} \quad \text{(at 30°)} \qquad (10)$$

The most striking factor in the action of methylene blue on zinc with hydrochloric acid is that it catalyzes hydrogen evolution, and 32 to 40 equivalents of zinc dissolve per mole of dye reduced. The rate is closely first-order, which indicates that the catalysis is proportional to the concentration of the blue and not the white form. A typical experiment is shown in TABLE 4. After the color disappears, the rate drops to one or two mg. per 10 minutes, and the value of *a* used in the rate equation is the total zinc dissolved up to this point.

TABLE 4

THE RATE IN 75 ML. 0.105 M HCL, 0.002 M METHYLENE BLUE AT 25°, 3150 R P M.

Time, min.	Zn dissolved, mg.	k
6	30 3	0.118
8	34.4	.125
10	35.6	.122
12	33 6	.127
12	26 9	.127
20	32 2	.130
∞	(206)	

Attempts were made to measure a rate constant colorimetrically. However, these were impeded by the cloud of tiny hydrogen bubbles which remains suspended a long time, and by reoxidation of the methylene white. With improved technique, the measurements should be possible.

TABLE 5

THE RATE IN VARIOUS SOLUTIONS, 25°, 3150 R.P M., 75 ML. SOLUTION

Acid	$C_{\mathrm{Me\ blue}}$	Time min	Zn dissolved, mg.	E
0.108 M HAc	0.01	4	2.8	0.08
.10 M NaAc	——	4	2.5	
0.108 M HAc	.004	20	16.0	
.10 M NaAc	——	20	19 4	
0.1 M HAc	.004	20	33.6	.25
	——	20	24.0	
0.1 M HAc .04 M HCl	.002	k	~ 0.103	.40
0.105 M HCl	.002	20	73	.45
	——	20	37	

The experiments of TABLE 5 were run with a zinc cylinder which dissolved undesirably fast in acids alone. However, they indicate that methylene blue does not act as a depolarizer at all in an acetic acid buffer of pH ~ 5, and that the rate is intermediate at lower pH values. A zinc cylinder was rotated 110 minutes in the acetic acid buffer with 0.004 M methylene blue; it lost 62 mg. and became rough, pitted and considerably stained, but the solution lost little color. With 0.1M HCl, decolorization is almost complete in 80 minutes.

The catalysis of hydrogen evolution is probably connected with the fact that reduction of methylene blue requires two electrons but only one hydrogen ion per molecule:

$$C_{16}H_{18}N_3S^+ + H^+ + 2\varepsilon \rightarrow C_{16}H_{19}N_3S \qquad (11)$$

The experiments reported here suggest that the metal is primarily attacked by the hydrogen ion, that the dye is absorbed and lowers the potential barrier in the over-all process

$$2H^+ + 2\varepsilon \rightarrow H_2$$

Obviously, further experiments with other oxidation-reduction systems are desirable, in strongly acid and buffered solutions and with pure zinc which has a lower spontaneous rate. A few experiments with indigo disulfonate indicate that it has a rather small depolarizing action, with $E \sim 0\ 27$ volt.

Note A special lecture was held on April 28, 1948, at which Doctor Jean Pasteels, Laboratoire d'Anatomie et Embryologie Humaines, Université Libre de Bruxelles, Bruxelles, Belgium, presented a paper entitled *The Evolution of Nucleic Acids in the Germinal Cycle of* Ascaris megalocephala, *as studied by Cytochemical Methods*, illustrated by lantern slides This will be published in Volume 11 of TRANSACTIONS

CONFERENCES HELD

SECTION OF PHYSICS AND CHEMISTRY

CONFERENCE ON "MOLECULAR INTERACTION": APRIL 9 AND 10, 1948

The Section of Physics and Chemistry held a Conference on "Molecular Interaction." Doctor R. M. Fuoss, Yale University, New Haven, Connecticut, was the Conference Chairman in charge of the meeting. Doctor P. Debye, Cornell University, Ithaca, N. Y., was the Presiding Chairman at all sessions.

The program consisted of the following papers:

Friday, April 9

Morning Session. "Introductory Remarks," by the Conference Chairman.

"Colloidal Electrolytes," by P. Debye, Cornell University, Ithaca, N. Y.

"The Role of Hydration in the Debye-Hückel Theory," by R. A. Robinson, Yale University, New Haven, Connecticut, and R. H. Stokes, Cambridge University, Cambridge, England (presented by title).

"The Brönsted Work Principle: Thermoelectric, Galvanic and Thermo-Chemical Cells," by O. Foss, The Rockefeller Institute, and V. K. LaMer, Columbia University, New York, N. Y.

"The Effect of Shape on the Interaction of Lyophilic Colloids," by L. Onsager, Yale University, New Haven, Connecticut.

"The Attractions of Proteins for Small Molecules and Ions," by G. Scatchard, Massachusetts Institute of Technology, Cambridge, Massachusetts.

Afternoon Session. "Determination of the Dissociation of Constants of Weak Electrolytes in Salt Solutions," by M. Kilpatrick, Illinois Institute of Technology, Chicago, Illinois.

"Kinetics of the Formation of Monodisperse Sulfur Sols," by V. K. LaMer and E. M. Zaiser, Columbia University, New York, N. Y.

"Thermal Properties of Electrolytic Solutions and the Debye-Hückel Theory," by F. T. Gucker, Jr., Indiana University, Bloomington, Indiana.

"Interactions Between Water Molecules and High Polymers," by M. Dole and I. L. Faller, Northwestern University, Evanston, Illinois.

"The Scientific Contributions of Grinnell Jones," by M. Dole, Northwestern University, Evanston, Illinois (presented by title).

Saturday, April 10

Morning Session. "Pressure-Volume-Temperature Relations in Some Solutions of Electrolytes," by R. E. Gibson, Applied Physics Laboratory, Silver Springs, Maryland.

"Extrapolation of Apparent Molal Properties of Strong Electrolytes," by B. B. Owen and S. R. Brinkley, Jr., Yale University, New Haven, Connecticut.

"The Surface Tensions of Aqueous Solutions of Sulfuric Acid," by T. F. Young, University of Chicago, Chicago, Illinois.

"Diffusion Coefficient of Potassium Chloride," by H. S. Harned and R. L. Nuttall, Yale University, New Haven, Connecticut.

Afternoon Session. "Some Interactions in Solutions of Electrolytes," by C. A. Kraus, Brown University, Providence, Rhode Island.

"Dielectric Absorption by Ammonium Salts in Solvents of Low Dielectric Constant," by R. H. Cole and H. A. Strobel, Brown University, Providence, Rhode Island.

"Factors Influencing the Properties of Long Chain Electrolytes in Water and in Mixtures of Water and Other Solvents," by P. F. Grieger, Brown University, Providence, Rhode Island.

"The Viscosity of Mixtures of Polyelectrolytes and Simple Electrolytes," by R. M. Fuoss and U. P. Strauss, Yale University, New Haven, Connecticut.

NEW MEMBERS

Elected April 22, 1948

SUSTAINING MEMBERSHIP

Beube, Frank E., L.D.S., D.D.S. Assistant Clinical Professor; Acting Head, Division of Periodontia, College of Dentistry, Columbia University, New York, N. Y.

ACTIVE MEMBERSHIP

Adelson, Harvey T., Student, Biochemistry, Polytechnic Institute of Brooklyn; Research, Pediatrics Research Laboratory, Jewish Hospital of Brooklyn, Brooklyn, N. Y.

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Borenfreund, Ellen, B.A., Cancer Research. Student, Department of Biology, New York University, New York, N. Y.

Bowman, John R., Ph.D. Head, Department of Research in Physical Chemistry, Mellon Institute, Pittsburgh, Pa.

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Brotman, Irving, M.D. Washington, D. C.

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Cooper, Jack, Ph.G. Chief Research Pharmacist, Ciba Pharmaceutical Products, Inc., Summit, N. J.

Denes, Gyula. Director, Psychodramatic Theatre, New York, N. Y.

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Dolores, Emeterio P., M.S. Instructor in Biology, Chemistry, Zoology, College of Oral & Dental Surgery, San Beda College, Far Eastern University, Manila, Philippines.

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SECTION OF BIOLOGY

May 10, 1948

SYMPOSIUM: AMINO ACID EXCRETION

RENAL CLEARANCES OF ESSENTIAL AMINO ACIDS

By LEMUEL D. WRIGHT*

Protein hydrolysates or amino acid mixtures are being employed widely today in clinical medicine. Present practices of administering these preparations either orally or parenterally in relatively large amounts have emphasized the need for additional information concerning the extent to which the various essential amino acids are lost in the urine.

There are a variety of approaches to the problems of the excretion of amino acids in the urine. We have selected to employ, in the studies to be reviewed in this paper, certain comparatively recently devised techniques of renal physiology whereby, under a variety of experimental conditions, the fate of the amino acids within the kidney may be fairly well traced. For a discussion of the underlying theory and technique involved in this field of research, reviews by Smith and others[1, 2] may be consulted.

In the experiments to be described, trained female dogs were employed. Infusion of the amino acids at definite rates was accomplished by the use of an electrically driven apparatus permitting constant rates of infusion. The amino acids were administered *via* the radial vein. Creatinine in amounts sufficient to permit the determination of reliable creatinine clearances was administered subcutaneously in aqueous solution. Blood samples were obtained from the femoral vein. All urine samples were obtained by catheterization. The bladder was washed out with a definite volume of water prior to and following the collection of urine samples in order to obtain quantitative urine collections.

Microbiological methods of assay have been employed in the determination of plasma levels and urine concentrations of the various amino acids studied. Determinations of the amino acids in plasma were carried out on protein-free filtrates prepared according to the method of Dunn *et al.*[3] Assays on suitably diluted urine samples were carried out without previous treatment. The method of Stokes *et al.*[4]

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* Medical Research Division, Sharp & Dohme, Inc., Glenolden, Pa.

Note. The Section of Geology and Mineralogy held a meeting on May 3, 1948, at which Professor Daniel T. O'Connell, Department of Geology, College of the City of New York, New York, N. Y., presented a paper entitled *Illustrations of Cloud Types*, illustrated by Kodachrome slides. No abstract of this has been received.

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TABLE 1

RENAL CLEARANCE STUDIES ON *l*(−)-TRYPTOPHANE

Dog 84, wt. 15.9 kg.

Time	Creatinine clearance	Urine flow	*l*(−)-Tryptophane				
			Plasma conc.	Amount filtered	Amount reabsorbed	Amount excreted	Clearance
Control: Post-absorptive state, but after priming dose of water							
hr. :min.	cc./min.	cc./min.	mg./cc.	mg./min.	mg./min.	mg./min.	cc./min.
0:10	56.7	2.40	0.014	0.76	0.76	0.002	0.18
0:20	63.9	2.95	0.014	0.83	0.83	0.002	0.19
Priming 0.75 mg./kg.—Maintenance 1.5 mg./kg./min.—Infusion 3 cc./min.							
0:45	52.8	2.80	0.051	2.69	2.68	0.009	0.19
0:55	49.0	2.40	0.063	3.09	3.07	0.015	0.24
Priming 0.75 mg./kg.—Maintenance 1.5 mg./kg./min.—Infusion 3 cc./min.							
1:30	44.3	2.25	0.091	4.03	4.00	0.033	0.36
1:41	46.3	2.08	0.118	5.46	5.42	0.045	0.39
Priming 1.0 mg./kg.—Maintenance 2.0 mg./kg./min.—Infusion 3 cc./min.							
2:12	46.0	1.82	0.164	7.54	7.47	0.072	0.44
2:23	48.8	1.64	0.158	7.71	7.64	0.075	0.47

employing *Streptococcus faecalis* R as the assay organism was used for the determination of arginine, histidine, methionine, leucine, isoleucine, and threonine. The method of Kuiken *et al.*[5] with *Lactobacillus arabinosus* was used for the determination of leucine, isoleucine, and valine. Lysine was measured with *Leuconostoc mesenteroides* P-60 according to Dunn *et al.*[6] Phenylalanine was determined with *Lactobacillus casei* by the procedure of Dunn *et al.*[7] Tryptophane was assayed with *Lactobacillus arabinosus* according to Greene and Black.[8]

The first phase of the work to be presented has been the determination of the renal clearances at normal and elevated plasma levels of each of the ten amino acids loosely referred to as "essential." In these initial experiments involving clearances of the individual amino acids, the plasma levels of each amino acid have been elevated to at least 10 times that of the fasting condition. It was reasoned that this level would be higher than any value possible to obtain following the administration of the amino acid in protein digests or complete amino acid mixtures. Subsequent investigations, to be discussed later, have demonstrated the correctness of this assumption.

Renal clearance data with tryptophane are shown in TABLE 1. The experiment described lasted about 2½ hours and involved the determination of tryptophane clearances in the post-absorptive animal, followed by similar clearances at various elevated plasma levels of tryptophane. Two calculations that are interrelated in their derivation, namely, the amount of tryptophane reabsorbed by the tubules in proportion to that filtered at the glomerulus and the clearance of tryptophane in comparison to that of creatinine, serve to demonstrate that tryptophane when administered alone is well reabsorbed by the renal tubules at all plasma levels feasible for study.

The amino acids leucine, isoleucine, valine, histidine, methionine, phenylalanine, and threonine have been examined in a similar fashion and resemble tryptophane in that tubular reabsorption is essentially complete following glomerular filtration from plasma levels up to at least 10 times that of the fasting state.[9-11]

TABLE 2

RENAL CLEARANCE STUDIES ON $l(+)$-ARGININE

Dog 84, wt. 16.1 kg.

Time	Renal plasma flow PAH	Creatinine clearance	Urine flow	$l(+)$-Arginine				
				Plasma conc.	Amount filtered	Amount reabsorbed	Amount excreted	Clearance
Control: Post-absorptive but after priming dose of water								
hr.: min.	cc./min.	cc./min.	cc./min.	mg./cc.	mg./min.	mg./min.	mg./min.	cc./min.
0:10	231.7	79.5	2.90	0.031	2.46	2.46	0.004	0.13
0:20	216.0	70.2	3.85	0.018	1.26	1.25	0.005	0.33
Priming 2.0 mg./kg.—Maintenance 2.0 mg./kg./min.—Infusion 3 cc./min.								
0:50	205.8	77.8	4.90	0.112	8.71	8.70	0 010	0.09
1:00		78.3	4.90	0.153	11.98	11.96	0.017	0.11
Priming 3.0 mg./kg.—Maintenance 6.0 mg./kg./min.—Infusion 3 cc./min.								
1:30		68.1	5.60	0.338	23.02	12.03*	10.99	28.3
1:40	177.5	57.8	5.80	0.458	26.47	12.80*	13.67	29.8
Priming 4.0 mg./kg.—Maintenance 10 mg./kg./min.—Infusion 3 cc./min.								
2:10	174.6	60.7	6.64	0.680	41.28	10.75*	30.53	44.9
2:20		63.6	6.78	0.720	45.79	8.42*	37.37	51.9

* Tm = 12.02, 12.80, 10.75 and 8.42 (average 11.00) mg./min.

TABLE 2 summarizes renal clearance studies with arginine. As the plasma level of this amino acid was elevated, resulting in commensurate increases in the amount of amino acid filtered, there was encountered a plateau in the amount of the compound that may be reabsorbed amounting to about 11 mg./min. This value is spoken of as maximal rate of tubular reabsorption or Tm.[10]

The amino acid lysine is quite similar to arginine in being poorly reabsorbed following filtration from elevated plasma levels. The Tm for lysine is approximately 13 mg./min. for a dog of about 15 kilos.[10]

This phase of the work may be summarized by saying that eight of the essential amino acids leucine, isoleucine, valine, methionine, histidine, threonine, phenylalanine, and tryptophane are reabsorbed well at elevated plasma levels while two, arginine and lysine, are poorly reabsorbed at elevated plasma levels.

Having demonstrated that the individual essential amino acids vary with respect to the extent to which they are reabsorbed by the kidney following filtration from elevated plasma levels, it has been of interest to inquire what effect, if any, the reabsorption of one essential amino acid may have on the reabsorption of another related or unrelated amino acid. The next phase of this discussion describes certain studies wherein various *pairs* of amino acids were administered together and their competition for reabsorption by the renal tubules investigated.[12] These studies have involved the coadministration of (a) two amino acids *well* reabsorbed at high plasma concentration, (b) two amino acids relatively *poorly* reabsorbed at high plasma levels, or (c) two amino acids one of which was *poorly* reabsorbed and the other *well* reabsorbed when each was administered alone.

TABLE 3 illustrates an instance of competition for reabsorption between the amino acids arginine and lysine. In the first phase of the experiment, the effect of arginine, at filtration loads sufficient to measure its Tm, on the reabsorption and clearance of endogenous lysine was studied. In the second phase of the experiment, the mutual effect of the two com-

TABLE 3

COMPETITION BETWEEN ARGININE AND LYSINE FOR TUBULAR REABSORPTION

Dog 365, wt. 13.9 kg.

Creatinine clearance	Urine flow	*l*(+)-Arginine				*l*(+)-Lysine			
		Plasma conc.	Amount filtered	Amount reabsorbed	Clearance	Plasma conc.	Amount filtered	Amount reabsorbed	Clearance
Arginine dosage: Priming, 4.0 mg./kg., i.v.; maintenance, 7.0 mg./kg./min., i.v. for the duration of the experiment. Infusion rate, 3.0 cc./min. Equlibration period of arginine infusion before initial clearance, 25 min.									
cc./min.	cc./min.	mg./cc.	mg./min.	mg./min.	cc./min.	mg./cc.	mg./min.	mg./min.	cc./min.
67.7	7.7	0.31	20.85	9 67	36.29	0.048	3.25	1.75	31.3
67.0	8.2	0.38	25.73	9.46	42.37	0.051	3.42	1.93	29.2
Lysine dosage: Priming, 4.0 mg./kg., i.v.; maintenance, 7.0 mg./kg./min., i.v. Arginine dosage: Maintenance, 7.0 mg./kg./min., i.v. Equilibration period of arginine plus lysine infusion before next clearance, 25 min.									
58.0	10.4	0.54	31.15	3.05	52.82	0.40	23.03	3.13	50.10
59.7	10.3	0.61	36 60	5.10	51.38	0.46	27.70	2.64	54.00

pounds at elevated plasma concentration on their respective reabsorption capacities was studied.

The results of TABLE 3 show that the reabsorption of arginine at elevated plasma concentration, or filtration load, suppressed the tubular reabsorption of endogenous lysine significantly. This effect is the more apparent if it is recalled that, at an endogenous plasma concentration of lysine alone, its clearance was 0.14 cc./min. as compared with clearances of about 30 cc./min. in the present study at similar plasma concentrations.

At elevated plasma concentrations of arginine and lysine simultaneously administered, Tm values of 3-5 mg./min. for arginine and 2-3 mg./min. of lysine were encountered. If it is recalled that the Tm for lysine administered alone is about 13 mg./min., the conclusion seems justified that the reabsorption of one amino acid of this pair results in impaired reabsorption of the other member. In passing, it might be mentioned that it has been possible, in at least one experiment, to inhibit lysine reabsorption completely by the administration of arginine. In other words, all the lysine filtered by the glomerulus was eliminated in the urine.

Other instances of competition of amino acids for reabsorption or interference of one amino acid with tubular reabsorption of another amino acid have been noted. This phenomenon has not been observed, however, with all the amino acid pairs investigated. Apparently, there exist certain pairs of amino acids where competition for reabsorption is demonstrable and certain pairs of amino acids for which no competition can be shown. We have attempted to correlate this information in terms of what it may mean with respect to mechanisms by which amino acids in general are reabsorbed by the kidney tubules. We have postulated that those amino acids which can compete with each other for reabsorption are reabsorbed by a common mechanism. Furthermore, where competition for reabsorption cannot be demonstrated for two amino acids, it would follow that these two amino acids belong to separate groups with respect to mechanisms of tubular reabsorption. FIGURE 1 summarizes the results of such an interpretation. One reabsorption mechanism appears to be concerned with reabsorption of the basic amino acids arginine, histidine, and lysine. A second reabsorption mechanism may be postulated to exist for at least the monoamino-monocarboxylic amino acids leucine and isoleucine. Apparently, at least a third

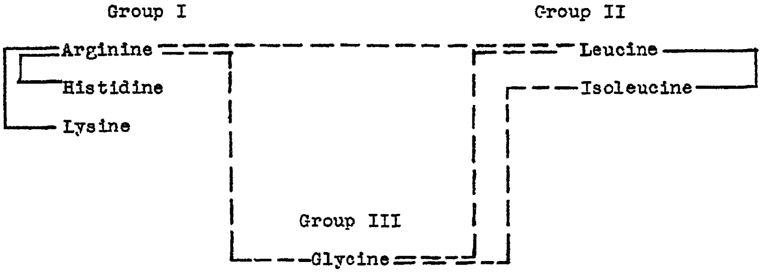

FIGURE 1 Classification of amino acids into groups with respect to probable mechanism of reabsorption. Solid lines indicate established interference; broken lines indicate no demonstrable interference.

mechanism of reabsorption exists, for it has been impossible to demonstrate a relationship between glycine and members of either group I or II.

It is obvious from our knowledge of the biological value of proteins that the administration of single amino acids or incomplete amino acid mixtures, except in certain isolated instances, is of no practical value. A few experiments have been carried out on the renal clearances of certain amino acids following the intravenous administration of the ten essential amino acids or an acid hydrolysate of casein reinforced with tryptophane. It should be appreciated that the amount of amino acid mixture or protein hydrolysate that can be administered intravenously to an animal is quite low. Such preparations, when administered at too great a rate, usually cause severe nausea and oliguria. Accordingly, although we can administer an amino acid mixture or protein hydrolysate *in amounts as large as we have been able to do with single amino acids* because any *one* amino acid comprises only a small fraction of a complete protein, we have not been able in this way to elevate the plasma levels of various amino acids to those levels readily obtained by single amino acid administration.

TABLE 4

RENAL CLEARANCE STUDIES OF ARGININE AND THREONINE ADMINISTERED IN A MIXTURE OF THE TEN ESSENTIAL AMINO ACIDS

Dog 370, wt. 14.6 kg.

Creatinine clearance	Urine flow	*l*(+)-Arginine				*dl*-Threonine			
		Plasma conc.	Amount filtered	Amount reabsorbed	Clearance	Plasma conc.	Amount filtered	Amount reabsorbed	Clearance
Control: Post-absorptive but after priming dose of water									
cc./min.	cc./min.	mg./cc.	mg./min.	mg./min.	cc./min.	mg./cc.	mg./min.	mg./min.	cc./min.
75.5	6.6	0.020	1.55	1.54	0.40	0.038	2.94	2.92	0.43
69.2	4.0	0.017	1.18	1.17	0.65	0.032	2.21	2.19	0.52
Amino acid mixture dosage: Priming 4.0 mg./kg., i.v.; Maintenance 4.0 mg/kg./min. Infusion rate 6 cc./min. Equilibration period of infusion before initial clearance 30 min.									
76.1	3.8	0.026	1.98	1.96	0.61	0.059	4.49	4.45	0.71
78.9	3.4	0.032	2.52	2.50	0.61	0.069	5.44	5.39	0.78
Amino acid mixture dosage: Priming 8.0 mg./kg., i.v. Maintenance 8.0 mg./kg./min. Infusion rate 6 cc./min. Equilibration period of infusion before initial clearance 30 min.									
77.2	1.2	0.039	3.01	2.98	0.75	0.095	7.33	7.14	1.95
62.8	0.5	0.040	2.51	2.48	0.83	0.102	6.41	6.23	1.72

TABLE 4 summarizes an experiment wherein a mixture of the ten essential amino acids was administered in the indicated amounts. Arginine was selected for study because, from our studies of the renal clearances of the separate amino acids, arginine was found to have the lowest Tm and, consequently, might be expected to be the first amino acid to appear in the urine. Threonine was studied because it has been observed to be one of the amino acids excreted in greatest amount following filtration from a fasting plasma level. When the ten essential amino acids were administered at approximately the maximal tolerated amount, there was no indication that the maximal rate of tubular reabsorption of either arginine or threonine had been even approached.

TABLE 5

RENAL CLEARANCE STUDIES OF ARGININE AND THREONINE ADMINISTERED IN THE FORM OF ACID HYDROLYZED CASEIN PLUS TRYPTOPHANE

Dog 84, wt. 17.8 kg.

Creatinine clearance	Urine flow	*l*(+)-Arginine				*dl*-Threonine			
		Plasma conc.	Amount filtered	Amount reabsorbed	Clearance	Plasma conc.	Amount filtered	Amount reabsorbed	Clearance
Control: Post-absorptive but after priming dose of water									
cc./min.	cc./min	mg./cc.	mg./min.	mg./min.	cc./min.	mg./cc.	mg./min.	mg./min.	cc./min.
101.3	6.95	0.010	1.01	1.00	1.27	0.063	6.38	6.37	0.20
97.2	6.95	0.012	1.17	1.16	0.78	0.063	6.12	6.11	0.12
Casein dosage: Priming 4.0 mg./kg., i.v.; Maintenance 4.0 mg./kg./min. Infusion rate 3 cc./min. Equilibration period of infusion before initial clearance 30 min.									
90.5	6.80	0.013	1.17	1.16	1.02	0.066	5.97	5.95	0.34
90.4	7.10	0.017	1.54	1.52	1.27	0.072	6.51	6.47	0.48
Casein dosage: Priming 8.0 mg./kg., i.v.; Maintenance 8.0 mg./kg./min. Infusion rate 3 cc./min. Equilibration period of infusion before initial clearance 30 min.									
92.4	8.55	0.017	1.57	1.54	1.78	0.067	6.19	6.14	0.75
89.4	8.20	0.019	1.70	1.67	1.68	0.072	6.44	6.38	0.75

TABLE 5 summarizes an experiment where an acid hydrolysate of casein plus tryptophane was administered to a dog at the maximal tolerated dose. Here, as with the amino acid mixture, arginine and threonine were not eliminated in excessive amounts in the urine.

In summarizing this paper, the following conclusions may be made:

(1) Amino acids vary with respect to the efficiency with which they are reabsorbed following filtration from elevated plasma levels.

(2) Certain amino acids may compete with each other for reabsorption by the renal tubules. Such competition studies have indicated the existence of more than one functional mechanism involved in the reabsorptive process of amino acids.

(3) The intravenous administration of amino acid mixtures or protein hydrolysates to the intact animal does not result in plasma levels of the individual amino acids such that their maximal rate of tubular reabsorption is exceeded.

(4) Plasma levels of the various amino acids following the intravenous administration of amino acid mixtures or protein hydrolysates are too low for marked competitive inhibition of reabsorption to be encountered.

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THE UTILIZATION OF AMINO ACID MIXTURES BY DOGS

By R. H. SILBER, E. E. HOWE, AND C. C. PORTER*

Mixtures of 9 or 10 "essential" amino acids have been found to promote growth in weanling rats.[1] Recently, it has been demonstrated [2] that the same amino acids also satisfy the repletion requirements of protein-depleted adult rats. Rose and Rice [3] reported that the amino acid requirements of the adult dog are qualitatively similar to those of the weanling rat. The ten "essential" amino acids of Rose have also been found to satisfy the requirements of the adult dog for plasma protein production, nitrogen balance, and weight maintenance.[4]

When paired-feeding is not employed, the growth of rats supplied amino acids does not equal the growth of rats fed high-quality protein.[5] The superiority of protein is generally assumed to depend upon the presence of dietary factors other than amino acids in the protein.

The Growth and Maintenance of Dogs Fed Mixtures of Amino Acids, Free of the D-Forms and Peptides. A litter of six 12 week-old Beagle puppies, weighing about 3.5 kg. each, was divided into 3 groups—2 pups were fed a protein-free diet,[6] 2 received the same diet with 20 per cent amino acids added, and 2 received 20 per cent Labco casein in the diet. Paired feeding was employed.

For a period of 12 weeks, the amino acid-fed pups gained an average of 0.4 kg. per week, whereas the pair-fed casein control pups gained 0.43 kg. per week. The pups that received protein-free diet lost about 0.5 kg. each in 4 weeks and were then offered the amino acid diet. A gain in weight of 0.26 kg. per week resulted during the 12-week repletion period.

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Blood chemistry, urinary analyses, and bromsulfalein retention tests revealed only one significant difference between the pups fed casein or amino acids. The feeding of amino acids favored the production of serum albumin and liver protein, relative to casein feeding.

In the first 3 or 4 days of the experiment, the pups consumed their amino acid diets rapidly and vomited shortly thereafter. Subsequently, they learned to consume the diet in small portions and succeeded in retaining sufficient amounts for growth. It is important to note that, if the pups had not learned to consume the diet in this way, it might have been considered inadequate in the usual nutritional sense, although all essential factors were present.

Since both dogs and rats have shown evidence of a physiological disturbance (nausea or diarrhea) when amino acid mixtures or protein hydrolysates are given orally, it seems logical to assume that there may be a physiological limitation of the voluntary intake of amino acid-containing diets, and may therefore explain, at least in part, the inability of animals to grow as rapidly on amino acid diets as on protein diets supplied *ad libitum.*

The maintenance of adult dogs fed or injected with amino acids as the sole source of dietary nitrogen was also studied. Two adult dogs were fed 1.5 gm. of casein (200 mg. N) and 80 calories per kg. per day for a period of 64 days. Two other dogs received an equivalent amount of nitrogen in the form of an amino acid mixture, in the diet for 40 days and by vein for the subsequent 24 days. Two additional dogs served as negative controls and received a protein-free diet throughout the test period.

Body weights and nitrogen balance were maintained in the dogs fed either amino acids or casein. Blood chemistry and urinary analyses revealed only minor differences between the animals. However, as in the pup experiment, amino acid feeding favored serum albumin and liver protein formation, relative to casein feeding.

The Excretion of Amino Acids and Peptides by Dogs Fed Protein Hydrolysates and Amino Acid Mixtures Orally or Intravenously. Six preparations, designed for intravenous alimentation of man, were given orally or parenterally to dogs previously maintained at a low level of protein nutrition by the feeding of 120 mg. of lactalbumin nitrogen per kg. per day. Each preparation was administered to each of 3 or 4 dogs for a period of 4 days and urines were collected quantitatively for analysis. The excretion of amino acids, peptides, and total nitrogen was compared with the excretion found after the oral administration of an equivalent amount of lactalbumin nitrogen.

Four of the preparations contained 25 to 45 per cent of their amino acids in peptide form. Two contained no peptides. After oral administration, 0.3 to 4.3 per cent of the free amino acids and 2 to 10.5 per cent of the peptides were lost in the urine. Nitrogen balance experiments[*] in dogs showed that all preparations were approximately equal to casein in biological value, with 61 to 73 per cent of the nitrogen retained in the body after oral administration.

When the same preparations were infused intravenously at a rate of 2 mg. N per kg. per min., 5 to 20 per cent of the free amino acids and 18 to 45 per cent of the peptides administered were found in the urine. The dogs infused with the preparation which had the highest peptide content also excreted the largest fraction of peptides (45 per cent). Furthermore, all of the nitrogen of this preparation was lost in the urine, although it had been previously demonstrated that, after oral administration, its biological value (nitrogen retention) was equal to that of the other five preparations. Such a preparation would not necessarily be valueless for intravenous therapy in man, but the results emphasize the necessity of proving that a preparation designed for intravenous use is available by this route when given to the species for which it is intended.

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[*] The design of the nitrogen balance experiments was the result of a group effort (U.S P. Subcommittee, headed by Drs J. B. Allison and D. V. Frost) and will be reported as such at a later date.

When the daily intake of protein hydrolysate or amino acid mixture was infused very rapidly (15 min.), only 25 per cent of the nitrogen was retained. This was due to an increased excretion of urea and ammonia nitrogen, not to excessive loss of amino acids in the urine, as suggested by Elman.[7] A four-fold increase in the rate of infusion (2 mg. N per kg. per min. to 8 mg. N per kg. per min.) caused a moderate increase in the loss of free amino acids in the urine (5 to 15 per cent), but had no effect on the excretion of peptides (40 per cent). This suggests that the loss of peptides in the urine is not primarily a problem of kidney threshold—that certain peptides cannot be utilized after intravenous administration, whether given at slow or fast rates. In this connection, it is of interest that some of the peptides were not utilized even after oral feeding. These data support the finding of Christensen[8] that intravenously administered amino acids are more readily utilized than peptides.

The Relationship Between Intake and Excretion of Essential Amino Acids in Dogs. In this study, 3 different amino acid mixtures were infused into 2 normal dogs at rate of 6 mg. N per kg. per min. and the urines were assayed by the methods of Stokes[9] for the essential amino acids. The total nitrogen intake was 200 mg. N per kg., and this was the only source of dietary nitrogen supplied during the one-day experiment. Approximately 10 per cent of the amino acids administered were excreted in the urine.

After the first experiment, the amino acid mixture was altered on the hypothesis that a disproportionately large excretion of any one essential amino acid indicates that it is present in excess in the mixture, and, conversely, that a disproportionately small excretion indicates that it is present in an inadequate amount. The concept is essentially the same as that involved in vitamin saturation tests, but is applied here to the relative needs of the body for the various amino acids.

As shown in TABLE 1, it has been possible to greatly decrease or eliminate the differences between the proportions administered and those excreted. Thus, application of the above hypothesis apparently makes it possible to determine the composition of an amino acid mixture which, after infusion into normal dogs at a uniform rate, will be excreted in essentially the same proportions (or pattern) as in the mixture given.

If the capacity of the absorptive mechanism(s) of the renal tubules is exceeded in these experiments, one amino acid (or more) may tend to "spill" into the urine. If this occurs, it might not be possible to determine the proportion of that amino

TABLE 1

THE PROPORTIONS OF ESSENTIAL AMINO ACIDS IN THE MIXTURES AND IN THE URINES OF DOGS AFTER INFUSION

Amino acid	Experiment 1 Mixture, %	Experiment 1 Urine, %	Experiment 2 Mixture, %	Experiment 2 Urine, %	Experiment 3 Mixture, %	Experiment 3 Urine, %
Arginine	9.2	10.0	7.1	3.5	8.1	3.5
Histidine	4.2	9.9	2.1	5.7	1.0	1.4
Isoleucine	10.8	4.1	13.6	9.0	14.4	10.2
Leucine	24.3	12.4	31.4	25.6	33.2	34.2
Lysine	13.6	33.4	6.9	12.9	3.3	5.3
Methionine	8.5	3.0	9.9	6.9	12.0	11.6
Phenylalanine	12.1	4.8	11.9	10.5	13.1	8.8
Threonine	7.5	19.0	4.1	13.3	2.8	9.3
Tryptophane	1.3	0.3	1.8	1.2	2.0	2.1
Valine	8.5	4.2	11.0	11.6	10.0	13.3

acid which will be repeated in the urine, due to the low threshold. Threonine may be such an exception, since extrapolation of the values obtained with this amino acid indicate that threonine will be excreted in a larger proportion in the urine regardless of the proportion in the infused mixture. Another interpretation of this result is that threonine is not needed by the dog under these conditions, which seems unlikely.

The specificity of the assay procedures for amino acids in urine is another limiting factor. It may be necessary to revise these results as more knowledge of the assay methods is obtained. The excretion of ketoacids of the essential amino acids would not complicate the experiment if the microbial assays detect these forms.

Since low renal thresholds for certain amino acids could seriously limit their utilization by the tissues, experiments are now being designed to minimize the role of the kidney, that is, a lower intake and a lower rate of infusion are being employed. Furthermore, the dogs are maintained for two weeks at a basal level of protein nutrition prior to infusion for a period of 4 days.

It remains to be determined how accurately this approach reflects the relative amino acid needs of the animal. If the small fraction of the infused mixture which is excreted in the urine (about 10 per cent) has the same relative composition, or pattern, of amino acids as the infused mixture, it follows that the remaining 90 per cent, having the same relative composition, was metabolized in the body. This must reflect, at least in part, the needs of the animal under the conditions of the experiment.

Whether this approach reveals specific amino acid needs or merely an increased need for certain amino acids, it may be useful in the future in designing amino acid mixtures for specific types of patients. Thus, a study of this nature, if carried out in a group of similar patients, may lead to the "tailoring" of an amino acid mixture for such patients.

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URINARY EXCRETION OF INDIVIDUAL AMINO ACIDS ON NORMAL AND LOW PROTEIN DIETS

By STANLEY W. HIER*

The precise measurement of the quantities of amino acids excreted in the urine is complicated by a number of factors. First, the amino acids are present in conjugated and peptide forms as well as in free form. Second, numerous derivatives and metabolites of the amino acids are also excreted. Third, the presence of ammonia, urea, and salt may influence chemical or microbiological tests.

Our experience in this field has been limited to microbiological methods which we have applied successfully to blood plasma, spinal fluid, and lymph. The presence of urea in urine has created a

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special problem in the handling of this body fluid, particularly for the determination of the free amino acids present in normal urine. Dilutions of only 1-2 or 1-4 are permissible in some cases to measure the small amounts of free amino acids present. At this low dilution, the amount of urea will be between 5-12 mg./cc. of the assay sample. This concentration has a marked depressing effect upon the growth of test organism as measured by acid production. Part of this effect is due to the formation of ammonia upon autoclaving the media, since the pH is markedly increased as the concentration of urea is increased. However, urea as such, introduced aseptically, will also inhibit growth.

The values we report for free amino acids were obtained by using only those levels of sample which supplied less than 3 mg. of urea per tube. Nevertheless, one hesitates to state that these values are absolute, in view of the uncertainty as to the influence of the urea. We feel that the determination of excretion of the total amount of the individual amino acids excreted has a much higher degree of significance because the urea effect is eliminated. The urine is made 2 N with HCl and autoclaved at 15 at. pressure for 5 hours. For tryptophane, alkaline hydrolysis is employed. In this way, all the urea is eliminated and very good recoveries are obtained. The procedure is not particularly satisfactory for cystine, which is destroyed by acid hydrolysis. The tryptophane analysis also creates a problem, since the extent of racemization is unknown.

With these considerations in mind, I should like to present data which have been obtained in studies on the influence of diet on amino acid excretion.

On normal diets[1] (TABLE 1), it may be seen that the mean value for excretion of aspartic acid in free form is 1.3 mg./day, while histidine is excreted at the rate ot 188 mg./day. The amount excreted of the remaining amino acids shown falls in between. When the totals (free and combined) are considered, the range is from 20.3 mg./day for isoleucine to 351.4 mg./day for glutamic acid.

The range for individuals may be seen to be at least 3-fold or greater for each amino acid. Only a small portion of the glutamic acid and aspartic acid is excreted in free form, while practically all the histidine is excreted in free form (or at least microbiologically active form). The other amino acids fall intermediate between these in the relative amounts of

TABLE 1

URINARY EXCRETION OF "FREE" AND "COMBINED" AMINO ACIDS BY EIGHTEEN SUBJECTS ON NORMAL DIETS

(Reported as mg. per 24 hours)

	Free		Total		Combined		Per cent combined form of total	
	Range	Mean	Range	Mean	Range	Mean	Range	Mean
Arginine	11.0–36.2	21.3±6.9	12.2–45.0	23.7±7.9	0–13.8	6.6	0–52.9	24.2
Aspartic acid	0–23.6	1.3±0.6	87.4–258.8	164.5±46.7	82.7–258.8	163.2	82.1–100	99.2
Cystine	45.2–138.0	87.7±25						
Glutamic acid	0–63.7	35.8±19.2	102.4–769.5	351.4±151.4	72.6–708.7	315.6	79.0–100	89.8
Histidine	60.3–378.0	188.3±99.2	65.4–438.8	203.3±101.1	5.1–123.2	40.5	4.8–34.5	14.9
Isoleucine	0–20.1	5.9±4.5	11.8–33.4	20.3±5.5	3.9–30.3	14.4	18.3–100	71.0
Leucine	3.8–18.5	9.6±3.3	11.9–40.0	21.2±6.6	3.5–29.6	12.3	29.4–81.8	55.6
Lysine	18.2–88.2	33.6±16.9	35.6–166.0	73.2±29.4	15.4–77.8	43.0	23.4–74.8	57.1
Methionine	4.1–13.5	7.8±2.9	4.1–15.0	8.6±2.8	0.5–6.3	2.3	4.7–60.5	24.6
Phenylalanine	7.5–34.0	16.4±7.1	10.3–45.4	23.3±7.9	3.1–16.9	8.4	14.4–66.6	35.2
Proline	3.7–14.8	8.5±2.9	23.1–62.1	42.8±12.9	18.1–54.4	34.3	65.1–89.0	80.2
Threonine	12.4–49.5	24.4±10.9	14.8–84.4	53.8±19.5	18.8–53.8	31.3	41.7–67.9	58.0
Tryptophane	8.5–56.0	24.6±11.3	11.2–86.1	41.4±17.5	0.6–28.3	18.4	1.3–71.8	41.9
Tyrosine	10.5–43.9	20.8±1.7	23.4–100.3	52.5±18	5.9–60.3	31.7	25.2–75.1	60.4
Valine	0–7.5	4.5±2.2	11.0–30.0	19.8±5.6	6.1–28.2	15.3	55.4–100	77.3

free and combined amino acids excreted.

When the values for total amino acid excretion for individual amino acids obtained by us are compared to those of Dunn,[2] excellent agreement is obtained. The values reported by Steele[3] for free amino acids excreted on various diets do not agree as well. Part of this, we believe, is due to differences in diet, but principally the dilutions used by Steele are quite different from ours and, therefore, the influence of urea and other urinary constituents may vary.

It is evident, from our data as well as from those of Steele, that methionine is apparently excreted to a much smaller extent than cystine. Even though the egg protein diet supplies considerably more methionine than the soy bean diet, the excretion is the same on both diets for cystine and methionine. Apparently methionine is utilized or deaminated more readily than cystine.

After determining the normal excretion of amino acids, we were interested in determining the influence of low protein diets on excretion of total amino acids. TABLE 2 shows 3 diets fed to healthy normal young women for 11 days on Level B and D and 6 days on Level E. The protein in both B and D is a balanced mixture of meat, potatoes, oats and bread but given in limited quantity. Level E contained no protein except in the cookies. The results are shown in TABLE 3. In all cases except phenylalanine and tyrosine, there is a decrease in the excreted amino acids on the low protein diets as compared to normal diets. There is, however, no direct correlation between intake and amino acid excretion since the excretion on 0.2 gm. N/day is not different from that on 2.5 gm. N/day. This may be due to the limited duration of the experiment and need for longer depletion periods.

Sauberlich and Baumann,[4] in rat experiments, have shown that, in the rat, the amino acids excreted daily are proportional to the protein intake. They fortified the casein with cystine so that the quality of the protein was not involved, but only quantity. They also showed that the percentage of ingested amino acids excreted on the low protein intake is slightly

TABLE 2

FOOD INTAKE, PROTEIN CONTENT, AND CALORIC VALUE OF THREE DIETS (2.5, 1.3 AND 0.2 GM. NITROGEN) FED IN PROTEIN STUDY

Foods	Level B (2.5 gm N_2)			Level D (1.3 gm N_2)			Level E (low N_2)		
	Amt. of food gm.	Protein gm.	Calories*	Amt. of food gm.	Protein gm.	Calories	Amt. of food gm.	Nitrogen gm.	Calories
Apple sauce	100	0.2	80.0	100	0.2	80.0			
Lemon juice	100 ml.	....	33.0	100 ml.	....	33.0	100 ml.	....	33.0
Lettuce	30	0.4	5.4	30	0.4	5.4			
French dressing (vinegar and spice)	10 ml.	....	....	10 ml.	....	....			
Salad oil	10 ml.	....	90.0	10 ml.	....	90.0			
Beef-chuck	11	2.0	24.0	8.3	1.5	18.1			
Butter fat	4.4	....	39.6	3.4	....	30.6			
Cream (coffee)	64.2	1.9	133.5	48.6	1.4	101.1			
Potatoes	13.3	0.3	12.5	10.1	0.2	9.5			
White bread	91.0	8.4	237.5	68.9	6.3	179.8			
Oats, rolled	10.4	1.5	41.2	7.8	1.1	30.9			
Fondant	65.0	....	231.0	65.0	....	231.0	65.0	....	231.0
Stuart formula tablets, 2									
Cookies	276.7	....	1141.0	276.7	....	1141.0	276.7	....	1141.0
Total		14.7	2068.7		11.1	1950.4			1405.0

* Calculated values from: Bradley, Table of Food Values.

TABLE 3

EFFECT OF LOW PROTEIN DIET ON URINARY EXCRETION OF AMINO ACIDS
(Determined after hydrolysis)

Amino acid	Normal diet	2.5 gm. N/day (11 days)	1.3 gm. N/day (11 days)	0.2 gm. N/day (6 days)
Arginine	23.7	17.3	19.9	14.9
Aspartic acid	164.5	100	82.1	81.8
Glutamic acid	351.4	194	169	168
Histidine	203.3	106	75.5	97.0
Isoleucine	20.3	9 2	11.0	8.6
Leucine	21.2	14.9	15.6	13.6
Lysine	73.2	36.3	36.7	29.3
Methionine	8.6	4.5	3.9	3.7
Phenylalanine	23.3	26 6	29.3	20.7
Threonine	53.8	39.2	29.5	32.4
Tryptophane	41.4	9.3	6.3	6.4
Tyrosine	52.5	48.9	43.3	41.9
Valine	19.8	14.1	12.3	10.9

higher than on the high protein intake. The range is from about 0.3-1.58 per cent. In the mouse, however, the excretion is much higher, ranging from 3-9 per cent of the ingested protein. This is partly explained by the fact that more of the amino acids are excreted in peptide form by the rat than by the mouse. There is a 4-fold increase in amino N after hydrolysis in the rat, but only 2-fold in the mouse.

The work of Pearce[5] demonstrates the additional fact that, on a protein diet deficient in one amino acid, the excretion of all amino acids is higher than that on a normal good protein diet. This is due to the inability of the organism to utilize the inadequate mixture of amino acids.

Summary

Sixteen amino acids assayed microbiologically were found to be present in some or all of normal human urines studied in free form. All were found to be present after hydrolysis to release peptides or conjugates.

Aspartic acid is almost completely bound, while 90 per cent of the glutamic acid is bound or unavailable to the organism until urine is hydrolyzed. Histidine, arginine and probably cystine, on the other hand, are present in urine principally in the free form (75-85 per cent). The remaining amino acids are present in approximately equal amounts of the free and combined forms.

Total excretion of aspartic acid, glutamic acid, and histidine is highest (160-350 mg./day), while excretion of methionine is lowest (8.6 mg./day). The excretion of the other amino acids falls in between.

Human subjects excrete smaller amounts of the amino acids studied as protein intake is decreased. Phenylalanine and tyrosine appear to be exceptions to this rule. Similar results were obtained with rats and mice. However, if an inadequate protein is fed to rats, the excretion of other individual amino acids is increased. The excretion of amino acids by human subjects is quite low on normal diets, since, with the exception of histidine, less than 3 per cent of the ingested amino acids are excreted.

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THE URINARY EXCRETION OF AMINO ACIDS IN MAN

By RICHARD D. ECKHARDT, ARNOLD M. COOPER, WILLIAM W. FALOON, AND CHARLES S. DAVIDSON*

There is no adequate substitute for a well-balanced oral diet. However, occasions arise when a nutritious diet cannot be adequately eaten, digested or absorbed and parenteral protein hydrolysate therapy is indicated. Numerous reports have been published concerning the nutritional adequacy of protein hydrolysates, their clinical tolerance, and their freedom from antigenicity. However, few studies have dealt with the excretion of amino acids in the urine following infusions of protein hydrolysate in man.

The experiments reported here were undertaken to determine the extent of the renal loss of amino acids after their intravenous infusion in man. During the course of these investigations, the excretion of amino acids by normal subjects on *ad libitum* diets and on diets of varied protein content was determined and will be summarized. Further, observations on the excretion of amino acids following infusions of amino acids in patients with active cirrhosis of the liver and Wilson's disease will be compared with results obtained in normal subjects.

Excretion of Amino Acids by Normal Subjects Eating ad libitum. The 24-hour urines of 8 normal adult subjects eating *ad libitum* of diets estimated to contain an average of 105 grams of protein daily were assayed (microbiological method of Stokes *et al.*[1]) for the 8 amino acids "essential" for man[2] and for arginine and histidine. The results tabulated in TABLE 1 are in general agreement with other published values employing microbiological methods.[3-5]

The urinary excretion of alpha amino nitrogen (gasometric ninhydrin method of Van Slyke, MacFadyen, and Hamilton[6]) by the 8 normal subjects averaged 158 mg. for the 24-hour period, a higher value per hour (6.6 mg.) than that of fasting specimens, which averaged 4.3 mg. per hour. Thus, the food ingested by the subjects increased the fasting excretion of alpha amino nitrogen from 103 mg. (4.3 mg. x 24 hours) to 158 mg. daily. The loss of 55 mg. of alpha amino nitrogen after consuming 105 grams of protein represents excretion into the urine of only 0.4 per cent of the amino nitrogen ingested.* This

TABLE 1

URINARY EXCRETION OF 10 "ESSENTIAL" AMINO ACIDS AND OF "FREE" ALPHA AMINO NITROGEN IN THE NORMAL, IN LIVER DISEASE, AND IN WILSON'S DISEASE (*Diet ad libitum*)

	Normals (Av. 8 subjects) mg./24 hours	Liver Disease (Av. 7 patients) mg./24 hours	Wilson's Disease (Av. 5 patients) mg./24 hours
Arginine	9.5	10.6	46.6
Histidine	163	122	393
Isoleucine	13.8	4.5	24.4
Leucine	7.2	11.4	72.6
Lysine	46.8	28.0	218
Methionine	4.4	10.5	19.4
Phenylalanine	14.0	20.4	119
Threonine	20.9	26.4	261
Tryptophane	12.7	21.9	59.2
Valine	7.4	6.8	62.3
"Free" Alpha Amino Nitrogen	158 (120 to 199)	175 (84 to 258)	399 (204 to 640)

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* Thorndike Memorial Laboratory, Second and Fourth Medical Services (Harvard), Boston City Hospital, and the Department of Medicine, Harvard Medical School, Boston, Massachusetts.

* Assuming the food protein to contain 16 per cent total nitrogen, and the total nitrogen to be 80 per cent alpha amino nitrogen.

value is identical with that observed to follow the oral ingestion of 50 grams of whole casein.[7]

Excretion of Amino Acids by a Normal Subject Receiving Diets of Varied Protein Content. Since food by mouth produced only a moderate increase in the excretion of amino nitrogen above that excreted in the fasting state, the effect on the excretion of amino acids by varying the protein intake in the diet was investigated. A normal subject was given a diet adequate in calories and calculated to contain, respectively, 150, 75, and 0 grams of protein daily for each of three 8-day periods. It was observed that increases in the quantity of protein ingested only moderately increased the excretion of amino acids and of alpha amino nitrogen above that on the protein-free diet. No significant decrease in the amino acid excretion occurred during the protein-free period.

The excretion of "peptides" in the urine (determined as alpha amino nitrogen after hydrolysis of the urine) continued throughout the protein-starvation regime and represented about four times the excretion of "free" amino acids—a proportion of "peptides" to "free" amino acids similar to that excreted by this subject while receiving the diets containing 75 grams and 150 grams of protein.

The excretion in the urine of "free" amino acids and of "peptides" which occurred while no protein was fed must be considered endogenous in origin. This "minimum" excretion was increased by the ingestion of protein, although large fluctuations in the quantity of protein eaten resulted in only small changes in the quantity of amino acids excreted.

Excretion of "Peptides" Following Protein Hydrolysate Infusions. A normal subject given 75 grams of a partial protein hydrolysate ("Amigen") intravenously daily for four days as the sole source of nitrogen maintained weight and nitrogen balance. Of the nitrogen excreted daily during this period, 0.28 gram was "free" alpha amino nitrogen and 1.3 grams were "polypeptide nitrogen" determined as alpha amino nitrogen after hydrolysis of the urine. Since two-thirds of the total amino acids in this partial hydrolysate were "free" and one-third were "conjugated", the amount of polypeptide nitrogen excreted daily represented approximately 45 per cent of the polypeptide alpha amino nitrogen administered, while less than 5 per cent of the free alpha amino nitrogen given was lost. These observations confirm those of other investigators,[8] who further noted that, in contrast to the large loss by excretion in the urine of the peptides of "Amigen", those present in a partial hydrolysate of fibrin ("Aminosol") were lost to a much smaller extent.[9] Thus, there is considerable variation in the renal excretion of the polypeptides derived from different protein sources. However, following the infusion of a complete protein hydrolysate free of peptides (Merck*), no increase was observed in this laboratory in the excretion of "peptides" above that excreted on the control regime. Since "free" amino acids are lost to a smaller extent in the urine than are "peptides" following their intravenous infusion, a completely hydrolyzed protein would appear to be of greater nutritional value than a partially hydrolyzed protein.

Excretion of Amino Acids by Normal Subjects Following Amino Acid Infusions. Five hundred cc. of a 10 per cent solution of amino acids*[10, 11] (a complete acid hydrolysate of casein supplemented with tryptophane and containing 50 grams of amino acids) was infused without glucose to eight normal subjects while fasting at three rates of administration: approximately 2 mg., 6 mg., and 10 mg. of nitrogen per kilogram of body weight per minute, representing infusion times of approximately 1 hour, 20 minutes, and 10 minutes, respectively. The solution employed was a complete acid hydrolysate of casein supplemented with tryptophane and glycine, and essentially devoid of glutamic and aspartic acids. Each 500 cc. infusion supplied 47 grams of amino acids, 7.5 grams of total nitrogen, 6.0 grams of alpha amino nitrogen, and the following amounts (*l*-form) of the "essential" amino acids: 2.5 grams arginine, 1.6 grams histidine, 3.8 grams isoleucine, 7.8 grams leucine, 4.2 grams lysine, 3.2 grams methionine, 2.7 grams phenylalanine, 1.1 grams threonine, 0.45 grams tryptophane, and 3.1 grams valine. The content of the individual amino acids in the mixture infused was determined by microbiological assay.[1]

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* "Solution of Amino Acids Mixture, 10 per cent", developed and supplied by Merck and Company, Inc., Rahway, New Jersey. This product was used for all infusions reported in this paper unless otherwise indicated.

The blood alpha amino nitrogen values (gasometric ninhydrin method of Hamilton and Van Slyke[12]) averaged 3.6 mg. per 100 cc. of plasma initially, rose to 22.5 mg. per cent 5 minutes after the infusion, rapidly fell to 7.2 mg. per cent 1 hour later, and returned to within normal limits by 4 hours, although most values were slightly above the pre-injection level and averaged 3.9 mg. per cent.

The excess excretion in the urine of alpha amino nitrogen above the control (*ad libitum* diet) occurred within the first 4 hours following the infusion, averaged 9.3 per cent of that infused, and coincided with the return of the blood amino nitrogen to within normal limits. The loss of amino nitrogen in the urine increased little with more rapid infusions (TABLE 2). Two additional subjects, in whom the urine was collected more frequently in the post-infusion period, excreted approximately 90 per cent of the amino nitrogen lost within the first hour after the infusion was completed. During this period, the most rapid decline occurred in the blood amino nitrogen.

The excretion above the control of each of the 10 individual amino acids administered likewise occurred within 4 hours following the infusion, and none exhibited a delayed excretion over the next 20 hours. These values are shown in TABLE 2. From 0.2 to 14.1 per cent of the individual amino acids infused was excreted in the urine at the slow rate of infusion. Thus,

TABLE 2

RELATION BETWEEN RATE OF INFUSION AND QUANTITY OF AMINO ACIDS EXCRETED IN THE URINE IN THE 24 HOURS AFTER ADMINISTRATION OF 500 CC. OF THE 10% SOLUTION OF AMINO ACIDS TO 8 NORMAL ADULTS

	Slow rate (2 infusions)		Moderate rate (3 infusions)		Rapid rate (3 infusions)		Average (8 infusions)	
	mg.*	%**	mg.*	%**	mg.*	%**	mg.*	%**
Arginine	8.7	0.3	18.1	0.7	37.8	1.5	23.0	0.9
Histidine	206	12.9	190	11.9	231	14.4	209	13.1
Isoleucine	9.1	0.2	55.6	1.5	98.6	2.6	61.0	1.6
Leucine	45.7	0.6	172	2.2	281	3.6	182	2.3
Lysine	149	3.5	264	6.3	354	8.4	268	6.4
Methionine	34.4	1.1	88.7	2.8	169	5.3	105	3.3
Phenylalanine	43.1	1.6	112	4.2	175	6.5	119	4.4
Threonine	155	14.1	175	15.9	196	17.8	179	16.3
Tryptophane	22.7	5.7	25.7	6.4	33.8	8.4	28.0	7.0
Valine	42.1	1.4	105	3.4	196	6.3	124	4.0
Total 10 "essential" amino acids	716	2.3	1206	4.0	1772	5.8	1298	4.3
"Free" alpha amino nitrogen	469	7.8	556	9.3	645	10.7	568	9.5

* Mg. of amino acid or of alpha amino nitrogen in excess of that excreted while receiving the *ad libitum* diet (TABLE 1).

** Per cent of administered amino acid or of administered alpha amino nitrogen excreted in urine.

the subjects excreted fairly large percentages of administered threonine and histidine, intermediate percentages of lysine and tryptophane, and smaller percentages of the other amino acids. In contrast to the marked variation in the excretion loss of individual amino acids at the slow rate of infusion, their additional loss with more rapid infusions varied little, so that an almost equivalent additional percentage of each amino acid given was excreted.

The patterns of the 10 "essential" amino acids administered intravenously, excreted in the urine, and (by difference) retained in the body are shown in TABLE 3. The proportion of the "essential" amino acids in the mixture differed considerably from that excreted in the urine. Thus, for example, although arginine and histidine were present in similar proportions in the mixture infused (8.1 and 5.1 per cent of the total 10 "essential" amino acids, respectively), their pattern in the urine was altered so that proportionately much more of the 10 amino acids excreted was composed of histidine (16.1 per cent) than of arginine (1.8 per cent). Since only a small portion of the infused amino acids was lost in the urine (averaging 4.3 per cent for all 10 amino acids assayed), the pattern of the amino acids retained in the body resembled quite closely the pattern administered.

It is not possible to ascertain why each of the amino acids was not excreted in the urine in the same proportion as administered (TABLE 3). At least three explanations exist: (1) differences in the quantity of amino acids infused as compared to their requirements by the body; (2) differences in the renal clearance of amino acids; and (3) differences in the rate of removal of amino acids from the blood stream by tissues. With each increase in the rate of infusion, however, the additional excretion of all the "essential" amino acids more closely resembled the composition of the amino acids infused.

The "essential" amino acids in the infusion mixture were preferentially retained by the subjects, while the dispensable amino acids were more freely excreted in the urine. The greatest retention was observed at the slowest rate of infusion. Thus, although the 10 "essential" amino acids comprised two-thirds of the total amino acids infused, they contributed less

TABLE 3

COMPARISON OF THE PATTERNS OF THE 10 "ESSENTIAL" AMINO ACIDS ADMINISTERED, EXCRETED, AND RETAINED FOLLOWING THE INFUSION OF 500 CC. OF 10% SOLUTION OF AMINO ACIDS TO 8 NORMAL ADULTS

	Per cent of total of 10 "essential" amino acids		
	Given*	Excreted**	Retained†
Arginine	8.1	1.8	8.4
Histidine	5.1	16.1	4.6
Isoleucine	12.6	4.7	12.9
Leucine	25.7	14.0	26.4
Lysine	13.9	20.6	13.6
Methionine	10.6	8.1	10.7
Phenylalanine	8.8	9.2	8.7
Threonine	3.5	13.8	3.0
Tryptophane	1.5	2.2	1.4
Valine	10.2	9.5	10.3
Total	100.0	100.0	100.0

* Calculated from the quantity of amino acids given.
** Calculated from the quantity of amino acids excreted, averaged value of 8 infusions, TABLE 2.
† Calculated from the quantity of amino acids retained (quantity administered minus quantity excreted).

than one-third of those excreted in the urine. The 10 "essential" amino acids contributed approximately one-fourth of the total amino acids excreted by these 8 normal subjects eating *ad libitum* and by the normal subject consuming varied amounts of protein

Excretion of Amino Acids by Patients with Liver Disease. The average daily excretion of the "essential" amino acids in the urine of seven patients with severe liver disease (5 cirrhosis, 1 hemochromatosis, and 1 subacute yellow atrophy) eating *ad libitum* did not differ markedly from normal subjects (TABLE 1). In this small series, however, there was considerable variation among patients in the quantity of individual amino acids excreted. Four additional patients with active cirrhosis of the liver received 50 to 100 grams of the amino acid solution as the sole source of nitrogen intravenously daily for from 2 to 4 weeks. Positive nitrogen balance, maintenance of weight, decline in serum bilirubin, and clinical improvement occurred. The amino acid mixture was well tolerated. The loss of alpha amino nitrogen in the urine by the patients with liver disease was similar to that observed in normals following infusions of equivalent quantities of the hydrolysate. Furthermore, the post-infusion pattern of the "essential" amino acids excreted in the urine of the patients with cirrhosis was similar to that noted in normals. Hyperaminoacidemia and azotemia were not observed, nor was there a progressive increase in the urinary excretion of amino nitrogen. Previous studies in this laboratory[13] likewise demonstrated that the rate of removal of amino acids from the blood following the parenteral injection of a protein hydrolysate ("Parenamine") in patients with severe cirrhosis of the liver was little altered from that observed in normal subjects. It is concluded that intravenously administered amino acids are well tolerated by patients with severe liver disease, are metabolized and excreted in a manner differing little from normal, and may promote clinical improvement when administered as the sole source of nitrogen.

Excretion of Amino Acids in Wilson's Disease (Hepato-Lenticular Degeneration). Each of 5 patients with Wilson's disease eating *ad libitum* excreted greater quantities both of alpha amino nitrogen and of each of the "essential" amino acids in the daily urine than did normal subjects (TABLE 1).* The increased loss of amino acids in these patients was largely accounted for by an excessive rate of excretion during the fasting state. Thus, the hourly rate of excretion of alpha amino nitrogen in the fasting urines of patients with Wilson's disease averaged approximately 20 mg., in contrast to values of less than 5 mg. per hour for normal subjects. Although the patients' fasting plasma alpha amino nitrogen values were frequently slightly elevated, there was a lack of correlation between the level of amino acids in the blood and the increased excretion of amino acids in the urine which was always abnormal. Neither the oral ingestion of protein nor the intravenous infusion of the amino acid solution resulted in a greater excretion than observed for normals. The associated metabolic abnormalities observed in the Fanconi syndrome[15] were not present, nor did the liver disease appear to be responsible for the increased aminoaciduria. It is tentatively concluded that a renal mechanism of undetermined type is operative in Wilson's disease which allows increased spillage of amino acids in the fasting state but which normally accommodates increased loads of amino acids to the kidneys, as following the ingestion of food or the infusion of amino acids.

Oral and Parenteral Amino Acid Requirements. Minimal oral requirements of the "essential" amino acids will undoubtedly be found less than the parenteral requirements for several reasons. First, there is an increased excretion of amino acids when the parenteral rather than the oral route of administration is employed. Second, large single infusions of protein hydrolysates may be less efficient than divided oral feedings. Finally, it is more difficult to provide adequate carbohydrate to achieve maximum retention of protein by the parenteral route. Thus, it was observed that a protein hydrolysate which contained minimum amounts of phenylalanine maintained nitrogen equilibrium when administered orally, but was incapable of sustaining nitrogen balance when given parenterally until additional phenylalanine was provided.[7] The urinary loss of phenylalanine was greater both in absolute amount and in percentage of that administered when less than the minimum requirement of the amino acid was given and negative nitrogen balance resulted, than when sufficient of the amino acid was available for nitrogen equilibrium. The parenteral requirements for the other "essential" amino acids, also, are likely to be found greater than their oral requirements.

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* The finding of an increased excretion of amino acids in the urine of patients with Wilson's disease has been reported by others.[14]

Practical Considerations of Protein Hydrolysate Therapy. The renal loss of amino nitrogen following the parenteral infusion of amino acids may be as great as 28 times the loss from the administration of an equivalent amount of nitrogen as whole protein orally.[7] Such losses following intravenous infusion as compared to oral feeding appear great until it is recalled that, with the excretion of 0.4 per cent (0.2 grams protein) of 50 grams of whole protein given orally or 10 per cent (5 grams protein) of 50 grams of hydrolyzed protein injected intravenously, the body retains at least 45 grams of the protein administered. Although it was observed that the renal excretion of amino acids was greater when 100 grams rather than 50 grams of amino acids was administered intravenously, the largest quantity of amino acids lost was so small in terms of grams of protein that the body retained for metabolic purposes more than 85 per cent of the protein given. Thus, the administration of protein or amino acids orally or intravenously, slowly or rapidly, and in large or small quantity was followed by excretion in the urine of but a small portion of the amino acids given. The provision of an excess of the "essential" amino acids in a hydrolysate should compensate for their increased excretion in the urine when the parenteral rather than the oral route is employed.

The patient to whom protein hydrolysates are being administered is concerned more with the unpleasant side effects and the duration of the infusion than with his urinary excretion of amino acids. Because of the length of time often required to infuse 50 to 100 grams of amino acids and the frequency of nausea and vomiting, parenteral protein hydrolysate therapy has unfortunately been limited, because of both inconvenience and discomfort to the patient, and the constant vigilance required by the physician. Since these disadvantages were minimized by the complete protein hydrolysate with which the studies reported here were conducted,[16] parenteral protein hydrolysate therapy may become more practical and the physician may, if necessary, provide optimum feeding of protein entirely by the intravenous route.

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Note. The Section of Psychology held a meeting on May 17, 1948, at which Professor O. H. Mowrer, Psycho-Educational Clinic, Harvard University, Cambridge, Massachusetts, presented a paper entitled, *Some Studies in the Psychology of Identification.* This will be published in Volume 11 of TRANSACTIONS.

Transactions

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SECTION OF ANTHROPOLOGY

PRELIMINARY REPORT ON A TRIP TO ETHIOPIA

By WOLF LESLAU*

The Ethiopic language aroused the interest of Western scholars as early as the 16th century. Old Ethiopic or Geez, at present the liturgical language of the country, and Amharic, the national language, were investigated first. Little by little, the other Semitic languages as well as the Cushitic and Nilotic linguistic families used in Ethiopia were studied also. Semitic scholars were soon aware, though not sufficiently, of the importance of Ethiopic for the understanding of the Semitic languages. Of the eight Semitic languages—Geez (old Ethiopic), Tigre, Tigrigna, Amharic, Harari, Gurage, Argobba, and Gafat—some are well enough known, others insufficiently, still others are known only by name.

It was with the intention of filling out the gaps in our knowledge of Ethiopic and of gaining an insight into the structure of the whole group that I went to Ethiopia in October, 1946, and stayed there until July, 1947. The trip was made possible through a Fellowship of the Guggenheim Foundation, a grant by the Viking Fund and another by Mrs. Denyse Mosseri-Dreyfus. I take this opportunity to express my sincere thanks to these sponsoring institutions and individuals.

Anyone who intends to do research work in Ethiopia will have to start with Addis Ababa, the capital. There he will get acquainted with the central government and obtain its permission to travel around the country. It is the Ministry of Pen which issues instructions to the various Ministries, and particularly to the Ministry of Interior, to facilitate the trip of the research worker. I personally had the privilege of having been received by the Emperor to whom, in a fifteen-minute conversation held in French, I explained the purpose of my visit to his country. He showed great interest in my project of surveying the languages of Ethiopia and gave me his support in traveling through all the regions in which I was interested. As from him, I received everywhere the fullest and most cordial cooperation.

Living conditions in Addis Ababa are pleasant. Several hotels accommodate the traveler with room and board. The currency used in Ethiopia is the Ethiopian dollar (*berr*), its exchange being 40 American cents. In this connection, it is interesting to note that half a dollar is called a shilling. To give an idea of the cost of living for a traveler, I should like to mention that, in 1946-47, the price of board at the Imperial Hotel varied between 10-12 Ethiopian dollars, that is, $4-$5 a day. For a traveler intending to spend only a limited time in Addis Ababa, it would not pay to rent a house, for the maintenance of which a certain number of servants would be necessary.

The research worker, and especially the student of history, religion, and languages should also avail himself, in Addis Ababa, of the help of the Ethiopian scholars—some of them of traditional standards, others who, through Western contacts, have a critical approach to science—who will readily give information on problems of traditional history, religion, and literature of the country. The National Library, although it contains a limited number of manuscripts, and the Museum attached to it will be helpful for the study of things Ethiopian.

As for the anthropologist, the city itself with its houses, its markets, and its people will be his best teacher. He will be especially interested to investigate the impact of Western civilization on Ethiopia. He will notice the general trend toward modernization, but he will still see side by side men and women clad in the traditional dress called *shamma* or toga, and others dressed in a perfect Western style; local courts held in the streets and government courts held in modern buildings applying Western codes; church schools with their teaching of Old Ethiopic only, and government schools where thousands of boys and girls receive modern education; mules on the sidewalks, on one hand, and two-wheel carriages called *gari,* cars, buses and airplanes on the other hand; religious festivals of local character, and others where the foreign representatives are invited; he will still hear the old Ethiopic songs at weddings and in coffee-houses, and modern Amharic songs with Western influence over the radio and at the weekly representations of the Patriotic Association.

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* Asia Institute, New York, N. Y. This paper, illustrated by lantern slides and recordings, was presented at the meeting of the Section on May 24, 1948.

The linguist will be able to find, in Addis Ababa, linguistic informants of nearly all the regions of the country, but his work will be most productive if he goes to the particular region in the language of which he is interested. This will enable him to make anthropological investigations at the same time.

After having spent six weeks in Addis Ababa, I went to the region of Gurage, a province situated southwest of Addis Ababa (see FIGURE 1). The trip first took me to Wolqitte (over 90 miles on the Addis Ababa-Djimma road), and then a mule-ride of seven hours brought me to the town of Endeber, the capital of the tribe of Chaha. The mules and guides were put at my disposal by the local authorities of Wolqitte.

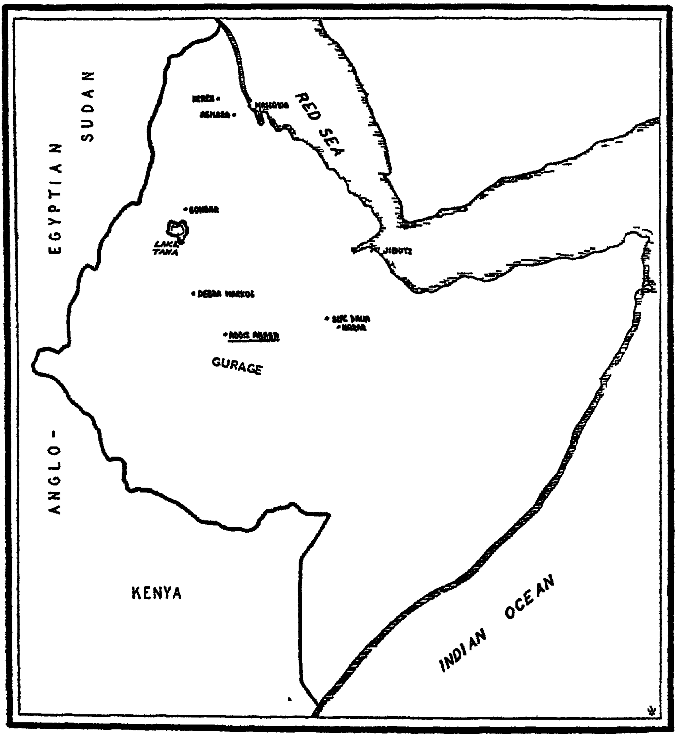


FIGURE 1.

The traveler will easily recognize the province of Gurage by the clean, round houses and by the characteristic banana-like plant called *ensat* in Amharic, generally known under the name of *musa ensete.* This plant provides the Gurage with flour; he makes cord out of the fiber, and the leaves of the plant also have various usages.

A few words on the country of Gurage, its people and languages might be useful to show the rich soil this province represents for the research worker.

Gurage is a relatively small province, and it is, therefore, all the more surprising to observe the great diversity of tribes, religions, and languages. It seems probable that the original population of the present-day Gurage was a Sidamo group from the South. The country was then occupied and settled by military colonies from the North of Ethiopia. Still other parts of the population might have come from the region of Harar. This situation is reflected in the ethnic, religious, and linguistic status of Gurage. In fact, there is a great variety of tribes, each of them divided into sub-tribes and families. The most important tribe seems to be that of Chaha. Concerning religion, the Gurage are generally Moslems, pagans, and Ethiopian-Christians. Some of them are Roman Catholics. As for the languages and dialects, their variety is as great as that of the tribes. We count about 5-6 main groups, each of them with a variety of dialects.

Endeber is the administrative chief-town of the Chaha section. It is inhabited by Gurage and Amharas. I was the guest of the native Catholic Mission there, which is situated at the outskirts of the town. The Mission has two sections, for boys and for girls. For my linguistic work, I had at my disposal boys from the age of ten to twenty-five living in the Mission, all of whom spoke Amharic beside their native tongue, while some of them knew French or English. My work consisted in collecting texts of anthropological and folkloristic character, material for a grammar of the dialect of Chaha, and a vocabulary of seven different dialects. I also had occasion to assist at religious festivals, and to travel around the country to observe the people at their work and leisure. Very often, I encountered young boys and girls who, eager to study, every day walk as much as three to four hours both ways to attend school at Endeber.

At this point, I should like to say a word about the food situation in the interior of Ethiopia as it concerns the traveler. If he can get used to pepper, which is the basic condiment of every kind of Ethiopian food, he will have no difficulty at all. Otherwise, he will have to carry food with him and have it cooked in his own way. I, myself, lived on chicken twice a day for seven weeks, besides eating the pancake-like native bread. The traveler will, of course, be careful to boil the water he uses.

After a stay of seven weeks in Endeber I fell ill and had to be carried down on a bed for seven hours to Wolqitte. From there, a car took me back to Addis Ababa. The illness was caused by fatigue. After a rest of two weeks in Addis Ababa, I went east to Harar, in order to investigate the language called *adare.*

The traveler can go to Harar either by train or by plane to Dire-Daua (265 miles), from where a bus will take him in about two hours (33 miles) to Harar. At the approach of the city, he will be agreeably surprised at the sight of a great variety of fruit and vegetables, such as bananas, oranges, lemons, papayas, and tomatoes.

Dire-Daua is the main rail center of the Djibouti-Addis Ababa line. The city has a European quarter occupied by French, Italians, Greeks and Armenians, besides the native quarter. The main body of the native population are Somalis and Gallas; the mosques of the city bring proof of the Moslem faith of its inhabitants. Since I had no particular linguistic interest in Dire-Daua, I spent only two days in the city, making recordings of Somali and Galla music.

The city of Harar presents a particular interest from a historical, anthropological, and linguistic point of view. Old Harar is a walled city with five gates. A little over an hour is sufficient to walk all around the walls. Even before the Italian occupation there were some buildings, governmental and private, outside the walls of the Old City. The Italians chose Harar as capital of the whole province and considerably extended this new section. The traveler who intends to spend some time in Harar will easily find a convenient house to rent, but he will welcome the good living conditions at the Grand Hotel.

Harar has attracted many students of history and anthropology, such as Paulitschke and Burton, and for good reasons. It was the capital of the Moslem-Ethiopian state and is still the center of Islamic culture in Ethiopia. Its inhabitants are mainly Gallas and Somalis of Moslem faith, but there are also Amharas. The student of religion and anthropology will be interested in studying the Arab life and culture in the surroundings of Gallas, Somalis, and Amharas. Everything indicates that Moslems live in the city: the seventy mosques, the bazars, the veiled women, the turbans of the men, the use of the narcotic plant (called *qat* in Arabic) in the morning hours, the particular style of the houses with nooks in the walls, the carpets, and the basket work. A city life of the Middle Ages can still be observed inside the walls of the city, bare of all vegetation. Arabic, Somali, Galla, Amharic, and the language particular to Harar called *adare* are spoken inside the walls, and very often by the same person.

I was particularly interested in the *adare* language. This is a Semitic language spoken only inside the walls, and with the growth of Amharic influence through the schools and the administration it might disappear. In Harar, I checked the linguistic material known through the various works, and collected new material for a grammar, a vocabulary of about 2,500 roots, texts in prose, and songs in Ancient Harari. The student of Harari will still find manuscripts of that language written in Arabic characters. I myself acquired a collection of songs in Ancient Harari in honor of the prophet Mohammed.

After a stay of six weeks in Harar, I returned to Addis Ababa to prepare another trip to Gojjam and Begemder, both north of Addis Ababa. The traveler can, of course, still go by mule to any of these regions, but he will be only too glad to go either by truck crossing the Blue Nile or by plane to Debra Markos, the capital of Gojjam (160 miles from Addis Ababa). He will reach Debra Markos by truck in approximately two days, by plane in forty minutes.

Ethiopian life can best be observed in the region of Gojjam. It is true that one notices here, too, the products of modernization—the airplane, the school, and the administration contribute their share to it—but the Ethiopian of Gojjam, and for that matter of Gondar, is still resistant to things which are not Ethiopic. Debra Markos is a perfect example of what was the ancient Ethiopian city. The palace of the Governor dominates the city, and the main means of transportation is the mule. The regional chiefs come from their provinces accompanied by their servants. The national feast, called *geber,* is still given in the traditional way, and at the end of it one will hear the noted *fukkara* or boasting of the warrior emitted in a particular modulation.

But Gojjam is more than all this. It is intimately linked with the whole religious and political history of Ethiopia. My interest in Gojjam was of a particular nature. In the region of the Blue Nile, there was a population called Gafat. This population had a language of its own, a Semitic language which was still understood, if not spoken, at the end of the 18th century. At that time, James Bruce had the Song of Solomon translated into various Ethiopic languages, including Gafat. All attempts by recent travelers to discover whether Gafat was still spoken remained in vain.

In fact, the inhabitants of Gafat at present speak Amharic, the national language of Ethiopia. On the basis of the only document on Gafat preserved in the Bodleian Library at Oxford, I worked out the structure of the language and published it, in 1945, together with the Gafat text and translation, in the *Gafat Documents.* Intrigued by a remark of the Abyssinian historian, Aleqa Tayye, in his *History of Abyssinia,* published in Amharic in 1927, that the inhabitants of Gafat still use their own language when they are among themselves, I tried to find out whether this language was still in existence. Dedjazmach Kebbede, the governor of Gojjam, took interest in my investigation. Not being a native of Gojjam, he was unable to give me information on the subject, but made an inquiry among the elders of the region. This investigation brought me to Burie, 60 miles north of Debra Markos.

The reader will understand my disappointment when, instead of the expected Gafat, I came across an artificial language which consisted in inverting the syllables, like saying *teb* instead of *bet.* Also, I was told that some judges in a native court knew Gafat, but when I came to see them I found out that, instead, they were interested in Gafat and wanted to study it from my *Gafat Documents.* Finally, after two weeks of inquiry, I found four people (three men and a woman, all of them old), from the region of Womberma in the vicinity of the Blue Nile who still spoke Gafat. I took them with me to Debra Markos, since the region of the Blue Nile is lowland and, consequently, very hot, and was able to investigate their language.

The linguist will be interested to know that, in my inquiry on Gafat, I came across special languages, such as the language of the merchants, the language of the chanters, and that of those possessed by the spirits. All these people speak Amharic, but use, for professional purposes, a more or less artificial language.

I left Gojjam after a stay of four weeks and went by plane to Gondar, the capital of Begemder. Gondar is situated to the north of Gojjam at a distance of 200 miles from Debra Markos. If Harar is the center of Islamic culture in Ethiopia, Gondar was, and in some respects still is (at least as tradition goes) the center of Ethiopian culture proper. It was the capital of Ethiopia in the 17th century. The various castles of the city, and of other places around Gondar, such as Old Gorgora on the Lake Tana, bear witness of the Portuguese in Ethiopia, who, as is well known, were asked by the kings to deliver the country from the Arab invasion in the 16th century.

Gondar is the city of old tradition in learning and Ethiopian scholarship. With its province, it is the center of numerous churches and monasteries. It has still preserved the old style in liturgical chanting, and the entire province has probably the finest preserved Ethiopic manuscripts and old Ethiopian paintings. Gondar is the most appropriate place for research in traditional history, liturgy, and religious problems. It is true that Ethiopian scholars are attracted by Addis Ababa, and Gondar is losing its value as a cultural center, but treasures of tradition still lie hidden in the churches and monasteries, and the priests and monks still can reveal much information on the ancient history of Ethiopia.

The student of linguistics will have an opportunity to examine the dialectal differences between the Amharic of Gondar and that of Shoa, and, besides, he will find himself in the center of the Cushitic languages of Agau. The student of things Ethiopian will not limit himself to a stay in Gondar, but will travel in the whole of the province of Begemder to see its monasteries and churches, and will in particular go for a boat ride on Lake Tana, the description of which would go far beyond this report. He will be rewarded not only by the literary treasures and relics of the ancient Ethiopians, but also by the sight of the green mountains, the deep valleys, and the numerous attractive islands of Lake Tana.

My main interest in Begemder lay in the investigation of the problem of the Falashas, that is, the Ethiopians of Jewish faith. The existence of Jews in Ethiopia was brought to the attention of the Western world as early as the 12th century. Beginning with the 18th century, various travelers visited the Falashas, but most of them had a missionary tendency which appreciably diminishes the usefulness and objectivity of their reports, so that I decided to concentrate my special attention on the Falashas. I left for Uzaba, a province to the southeast of Gondar, entirely inhabited by Falashas, and lived there for about three weeks, in the village of Ambworo.

I arrived at Passover—it was in the month of April—and, consequently, no one worked in the whole village. We gathered every day in the synagogue, priests, elders, and young men, and to the best of their knowledge they answered all my questions concerning their life. I visited all the neighboring villages and never came back from any of these visits without a goat, hens or eggs brought to me as gifts from all the Falasha huts. I also spent some days with the Falashas of the region of Seqelt to pay a visit to their monks. In this way, I gathered information on the most important phases of cultural, religious, and social life of the Falashas.

Not only did the Falashas receive me in the most cordial way I mentioned before, but nearly everywhere in Begemder the Ethiopians demonstrated their traditional way of hospitality. I shall never forget the reception in Gana Yohannes, in the region of Seqelt, where the governor of Seqelt came to receive me with fifty of his men and gave a copious *geber* in my honor.

In May, the small rains made their appearance and I went back to Addis Ababa. It is not an easy enterprise to travel in the interior during the rainy season. In some regions, it is even impossible to do it. I might add that, during the dry season which lasts from October to June-July, the climate of the highland is very pleasant: not too hot in the daytime, and cool at night. The same cannot be said of the lowland, where the heat is made more disagreeable by the presence of many flies. I intended to undertake a trip to the region of Ankober, to the north of Addis Ababa, to investigate Argobba, a language doomed to disappear. However, I felt the effects of fatigue and decided to work on Argobba with informants found in Addis Ababa. An Argobba Moslem woman, and occasionally a man, were my main informants, and with their help I gathered enough material for an analysis of the language.

In all the regions I visited, I made recordings of music, songs, and text specimens.

Before leaving Addis Ababa, I had the privilege to be received once more by the Emperor. This time, the conversation was in Amharic.

I left Addis Ababa in June, by bus for Asmara. The distance from Addis Ababa to Asmara is a little over 600 miles. I could, of course, have traveled by plane, but the trip by bus gave me the opportunity of crossing the whole of Ethiopia, the province of Tigre, and a part of Eritrea. The general impression the traveler gains from the northern part of Tigre and Eritrea is different from the remaining part of Ethiopia: Eritrea has a desert appearance. As for Asmara, it has the aspect of a European city; it also has a native section.

Besides the Cushitic and Nilotic languages spoken in Eritrea, the Semitic languages of Tigrigna and Tigre are used. Tigrigna being relatively well known, I concentrated my special efforts on the investigation of Tigre. To do this, I established myself in the city of Keren, 58 miles northwest of Asmara. Keren is a pleasant town, of a very "Oriental" aspect. I checked there the material previously published on Tigre and was able to supplement our knowledge in the domain of the grammar and vocabulary. Beside my linguistic work, I visited the various parts of the country, particularly Massawa, and also the monasteries of Debra Sina and of Debra Bizen, well known in the ecclesiastic history of Ethiopia.

Needless to say that the student of Ethiopia will be greatly interested in Eritrea if he is reminded of the fact that its population is half Christian, half Moslem; that the Italians lived in Eritrea for about fifty years and left there traces of Western culture; that ethnically the population is of mixed origin; that it has many interesting monasteries; that Semitic, Cushitic, and Nilotic languages are spoken there; and, finally, that the historical ties between Eritrea and the remaining part of Ethiopia are of the greatest importance for the future of the country.

The reader who knows something about Ethiopia will miss, in my description, many parts of the country not mentioned here. In fact, nothing or not enough was said about the Dankali, the Somali, the Galla, the Sidamo, the Agau populations, and others. The reason for not mentioning these provinces and populations is simply that, as I stated in the beginning, my interest for this trip was a survey of the Semitic languages of Ethiopia, and, consequently, I visited only those regions in which these languages are spoken.

If the student of Ethiopia keeps in mind the great amount of non-Semitic languages spoken there, the variety of populations and cultures, and, at the same time, the political unity of the country, he will be aware of the great interest Ethiopia offers to the student of history, religion, languages, anthropology, and the social sciences. These studies will throw light not only on Ethiopia, but also on Africa and on the present-day and ancient Near East. Whatever his particular field, the student will have satisfactory results in his investigation only if he has direct contact with the people. For keeping this up, he will need a speaking knowledge of Amharic, which is spoken and understood nearly all over Ethiopia. His work will be still more fruitful if he acquires a knowledge of the religious, cultural, and political past of the country.

In concluding, I should like to reiterate my thanks to the already mentioned institutions and individuals who made this trip possible; to the American officials, Mr. Felix Cole, formerly Minister of the United States to Ethiopia, and Mr. William H. Beach, Consul General, who manifested great interest and understanding for scientific work; to Prince Makonnen, Duke of Harar; to the many Ethiopian officials, and particularly to His Excellency Ato Aklilou Habte Wold, Vice-Minister of Foreign Affairs; Ato Akale Worq, Director of the Ministry of Education; Blatta Ayyele Gebre, Vice-Governor of Harar; Dedjazmach Kebbede, Governor of Gojjam; the Princess Tanagne Worq; and Bitwaddad Andarge, son-in-law of the Emperor and Governor of Begemder. Their cordial cooperation was extremely helpful in accomplishing my task. I also want to thank my many Ethiopian friends as well as the man in the street who helped me to feel and to understand Ethiopia. My gratitude goes particularly to His Majesty, the Emperor Haile Selassie, whose sincere interest and approval of my project provided me with the willing cooperation of the Ethiopian people.

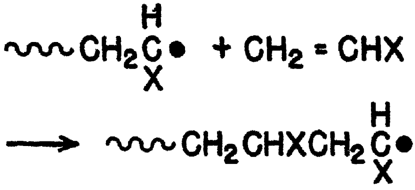
SECTION OF PHYSICS AND CHEMISTRY

COPOLYMERIZATION REACTIONS

By TURNER ALFREY, JR.*

Introduction. The tendency of many unsaturated organic compounds to form chain polymers is well known. While this polymerization reaction is by no means limited to vinyl compounds (vinylidene compounds, dienes, etc., also polymerize), the term "vinyl polymerization" will be used as a generic expression in certain parts of the discussion. When more than one monomeric material is present, the reaction is called "copolymerization," and the resulting product is called a "copolymer"

Vinyl polymerization is a chain reaction consisting of at least three steps: (1) chain initiation or activation; (2) propagation, or growth; and (3) chain termination. In the cases of thermal and peroxide-catalyzed polymerization, the activation step involves the formation of a free radical, and the propagation step consists of the addition of a monomer molecule to the growing chain without loss of the free radical activity.



$$\sim\!\sim CH_2\overset{H}{\underset{X}{C}}\bullet + CH_2{=}CHX \longrightarrow \sim\!\sim CH_2CHXCH_2\overset{H}{\underset{X}{C}}\bullet$$

On the other hand, when catalysts such as BF_3 are used, the reaction proceeds by an ionic mechanism. The latter type of polymerization will not be explicitly considered here, although many of the generalizations developed here should apply to ionic as well as free radical polymerization.

The complete kinetic analysis of a vinyl polymerization reaction is rather involved. There are a number of different possible mechanisms for both the activation and the termination steps. Furthermore, such additional processes as chain transfer take place under certain conditions. If more than one monomer is participating in the reaction, the problem obviously becomes still more difficult.

Fortunately, a considerable amount of information concerning copolymerization can be derived from consideration only of the *propagation* step of the reaction. The *rate* of copolymerization, and the molecular weight distribution of the resulting copolymer, depend, of course, upon the rates of initiation and termination as well as of propagation. The *chemical composition* of the copolymer, however, is determined almost entirely by the competition among the various propagation reactions. From a practical standpoint, this means that there is a simple equation (the "copolymer composition equation") which relates the composition of a copolymer to that of the monomer mixture from which it is formed. From a more fundamental standpoint, this relationship makes possible the quantitative study of the propagation step of polymerization.

The Copolymer Composition Equation:

$$\frac{dM_1}{dM_2} = \frac{M_1}{M_2} \cdot \frac{r_1M_1 + M_2}{r_2M_2 + M_1}$$

Consider the two vinyl monomers M_1 and M_2. Four propagation rate constants govern the growth of active polymer chains. These four reactions are summarized in TABLE 1. The symbols M_1^* and M_2^* refer to the concentration of active chain ends of the types M_1 and M_2.

In the steady state of copolymerization, $k_{12}M_1^*M_2 = k_{21}M_2^*M_1$. This indicates that, in the steady state, the rate of reaction of polymers ending in M_1 with monomer M_2 must be equal to the rate of reaction of polymers ending in M_2 with

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* Polytechnic Institute of Brooklyn, Brooklyn, N Y. This paper was presented at the meeting of the Section on February 3, 1948

The Section of Physics and Chemistry held a meeting on May 4, 1948, at which Professor Edward Saibel, College of Engineering and Science, Carnegie Institute of Technology, Pittsburgh, Pennsylvania, presented a paper entitled, *Applications of Chemical Reaction Rate Theory to Mechanical Phenomena,* which was illustrated. No abstract of this has been received

TABLE 1

Growing chain	Monomer adding	Rate of process	Reaction product
$—M_1^*$	M_1	$k_1(M_1^*)(M_1)$	$—M_1\text{-}M_1^*$
	M_2	$k_{12}(M_1^*)(M_2)$	$—M_1\text{-}M_2^*$
$—M_2^*$	M_1	$k_{21}\cdot(M_2^*)(M_1)$	$—M_2\text{-}M_1^*$
	M_2	$k_{22}\cdot(M_2^*)(M_2)$	$—M_2\text{-}M_2^*$

monomer M_1. It is now possible to express M_2^* in terms of M_1^*, although this approach cannot yield absolute values for M_1^* and M_2^*:

$$(M_2^*) = \left(\frac{k_{21}}{k_{12}}\right)\left(\frac{M_2}{M_1}\right)\left|M_1^*\right|$$

The rate of consumption of monomer M_1 is given by EQUATION 2:

$$-\frac{dM_1}{dt} = k_{11}(M_1^*)M_1 + k_{21}(M_2^*)M_1$$

Similarly, the rate of addition of M_2 to the polymer is

$$-\frac{dM_2}{dt} = k_{12}(M_1^*)M_2 + k_{22}(M_2^*)M_2$$

Finally, the ratio between the rate of addition of M_2 and the rate of addition of M_1 is given by:

$$\frac{dM_1}{dM_2} = \frac{k_{11}(M_1^*)M_1 + k_{21}(M_2^*)M_1}{k_{12}(M_1^*)M_2 + k_{22}(M_2^*)M_2}$$

Let us introduce the new variables r_1 and r_2.

$$r_1 = k_{11}/k_{12}$$

$$r_2 = k_{22}/k_{21}$$

EQUATION 4 can be rewritten in terms of r_1 and r_2.

$$\frac{dM_1}{dM_2} = \frac{M_1}{M_2}\cdot\frac{r_1M_1 + M_2}{r_2M_2 + M_1}$$

The ratio of the rates of addition of the two monomers is, of course, also an expression for the ratio of those two monomers in the resulting polymer.

$$\frac{m_1}{m_2} = \frac{M_1}{M_2}\cdot\frac{r_1M_1 + M_2}{r_2M_2 + M_1}$$

It is clear that the initial polymer, in general, does not possess the same composition as the monomer mixture, but is relatively richer in one component than in the other. The more reactive monomer is thus used up more rapidly, leaving the monomer mixture relatively poorer in this component. It follows necessarily that the last polymer to be formed is relatively richer in the less reactive constituent. The overall polymerization product, thus, is a mixture of copolymer molecules of different chemical constitution.

Distribution of Monomer Sequences. EQUATION 1 gives the *overall* molar composition of the copolymer molecules which form in a given monomer mixture. It is possible, however, to deduce a much more detailed picture of the copolymer structure from the rate constant ratios r_1 and r_2. Let us designate each group of successive M_1 units in the copolymer as a *sequence* of M_1 units. Each M_1 sequence is bounded on both ends by a sequence of M_2 units. The copolymer molecules consists of an alternating series of M_1 and M_2 sequences.

Depending on the relative values of the propagation rate constants, M_1 and M_2 sequences of varying sizes will be found in the polymer. When the rate constants k_{11} and k_{22} are large as compared to the rate constants k_{12} and k_{21}, one can expect to find long sequences of M_1's followed by long sequences of M_2's, whereas, if the relative values of those rate constants are reversed, then the polymer will contain short groups of M_1's followed by short groups of M_2's.

By defining four probabilities P_{11}, P_{12}, P_{21}, P_{22} for the probabilities of a monomer of the type M_1 adding to a chain ending in M_1^*, a monomer of the type M_2 adding to a chain ending in M_1^*, a mononer of the type M_1 adding to a chain ending in M_2^* and a monomer of the type M_2 adding to a chain ending in M_2^*, it is easy to calculate the distribution of lengths of M_1 and M_2 sequences.

To a chain ending in M_1^*, only one of two things can happen. (1) a monomer M_1 or (2) a monomer M_2 can add; therefore, the probability P_{11} will be the rate of addition of M_1 over the rate of addition of M_1 + the rate of addition of M_2, etc.

$$P_{11} = \frac{k_{11} M_1}{k_{11} M_1 + k_{12} M_2} = \frac{r_1 M_1}{r_1 M_1 + M_2}$$

$$P_{12} = \frac{k_{12} M_2}{k_{11} M_1 + k_{12} M_2} = \frac{M_2}{r_1 M_1 + M_2}$$

$$P_{21} = \frac{k_{21} M_1}{k_{21} M_1 + k_{22} M_2} = \frac{M_1}{M_1 + r_2 M_2}$$

$$P_{22} = \frac{k_{22} M_2}{k_{21} M_1 + k_{22} M_2} = \frac{r_2 M_2}{M_1 + r_2 M_2}$$

The distribution of sequence lengths (n_1) of M_1's and (n_2) of M_2's can be determined by the following consideration: To the first constituent M_1 of a group of M_1's picked at random, the addition reaction k_{11} (M_1^*) (M_1), whose probability is P_{11}, has had to be repeated $(n\text{-}1)$ times, to yield a group of nM_1's. The probability of these $(n\text{-}1)$ consecutive additions is $(P_{11})^{(n-1)}$. This sequence of reactions must then be followed by the addition of M_2, which has the probability P_{12}. The probability that a group of M_1's picked at random contains n_1 members is thus:

$$N(n_1) = P_{11}^{(n_1-1)} (1 - P_{11})$$

This is also the number distribution function for the lengths of M_1 sequences.

Similarly, the number distribution function for sequences of M_2's is given by:

$$N(n_2) = P_{22}^{(n_2-1)} (1 - P_{22})$$

where $N(n_2)$ represents the fraction of all M_2 sequences which possess n_2 members.

The average length of the M_1 sequences must, of course, bear a very simple relationship to the average length of the M_2 sequences, since there will be (neglecting end sequences) just as many M_1 sequences as M_2 sequences in the copolymer. If $\bar{n}_1$ and $\bar{n}_2$ are the number average lengths of M_1 and M_2 sequences, respectively, and M_1/M_2 is the number ratio of M_1's to M_2's in the molecule, then $\bar{n}_1$ and $\bar{n}_2$ are related by the expression:

$$\bar{n}_1 / \bar{n}_2 = M_1 / M_2$$

These M_1 and M_2 sequences are strung together in a *random order* in the copolymer molecule. In other words, the detailed structures of copolymer molecules formed from a given monomer mixture are those structures which would be formed if one took a container of M_1 sequences (distributed in length according to EQUA-

TION 9) and a container of M_2 sequences (distributed in length according to EQUATION 10), and alternatively picked out M_1 sequences (randomly) from one container and M_2 sequences (randomly) from the other container.

Speculations Regarding Effect of Structure on Reactivity with Free Radicals. In the previous section, it was demonstrated that the chemical composition and molecular structure of a polymer are determined by the competition of the two monomers for both types of free radicals (growing chain-ends). We now take up the important question: "How does the reactivity of a monomer in copolymerization depend upon its molecular structure?" The following generalizations can be made:

(1) Monomers which possess substituents on both carbon atoms of the C=C double bond (1,2-disubstituted ethylenes) exhibit a *specific reluctance* to add to *themselves* in the growing chain, although they may add to "vinyl" free radicals quite readily.

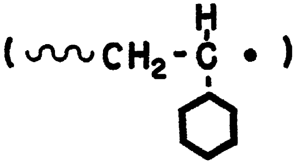
If, for example, M_1 represents styrene and M_2 represents diethyl maleate, the rate constant k_{22} is zero within experimental error, but the rate constants k_{11}, k_{12} and k_{21} all have finite values.

This is believed to be a *steric* effect.

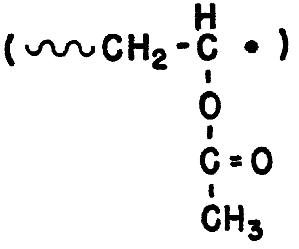
This leads to a number of interesting consequences. For example, these 1,2-disubstituted compounds exhibit practically no tendency to polymerize alone, but can often *copolymerize* readily with vinyl monomers. Maleic anhydride reacts with the styrene free radical about 20 times as fast as does styrene monomer (although maleic anhydride will not add to the maleic anhydride radical at all). Over a wide range of monomer ratios, the copolymer is almost exactly 50-50, with the structure -1-2-1-2-1-2-1-2-. This regular alternating structure represents the largest amount of M_2 which can be put into the copolymer, even with a large excess of M_2 in the monomer mixture.

(2) The chief factor governing the reactivity of a vinyl monomer is the amount of *resonance stabilization* of the *radical adduct* which is formed by the reaction of the monomer.

Thus, styrene exhibits a *strong* tendency to add to any given free radical, because the resulting adduct free radical is *stabilized by resonance:*



(This effect is identical with that which makes the triphenylmethyl radical so very stable.) Vinyl acetate, on the other hand, exhibits a *weaker* tendency to add to any given radical, because the resulting radical in this case is *not* stabilized by resonance:



When styrene and vinyl acetate must compete with each other for a given free radical, the styrene will be 30 to 50 times as reactive as the vinyl acetate.

(3) A second factor, of somewhat smaller importance, is the electrical *polarity* of the double bond. A substituent such as -CN, which is *m*-directing in the benzene ring, can be expected to *withdraw electrons* from the polarizable double bond, thus giving the double bond a + character. Similarly, such a group will give to a *free radical* a + polarity. Other substituents will be *electron-donating*, and will give a negative polarity to the double bond of the monomer and to

the carbon atom of the corresponding free radical. A free radical with a positive + polarity will exhibit a particular preference for a monomer with a negative double bond, and vice versa.

(4) A semi-quantitative scheme has been developed in which the *general reactivity* of a monomer (determined by the resonance effect) is denoted by the symbol Q, and the polarity is denoted by the symbol e. In this scheme, the copolymerization ratios are given by the equations:

$$r_1 = \frac{k_{11}}{k_{12}} = \left(\frac{Q_1}{Q_2}\right) e^{-e_1(e_1 - e_2)}$$

$$r_2 = \frac{k_{22}}{k_{21}} = \left(\frac{Q_2}{Q_1}\right) e^{-e_2(e_2 - e_1)}$$

This pattern of behavior arises from the hypothesis that the rate constant for the addition of a monomer of type 2 to a free radical of type 1 can be given to a good approximation by an expression such as the following:

$$k_{12} = P_1 Q_2 e^{-e_1 e_2}$$

P_1 is characteristic of radical 1; Q_2 is the mean reactivity of monomer 2; e_1 is proportional to the charge on the end group of radical 1; and e_2 is proportional to the charge on the double bond of monomer 2.

It would be a reasonable speculation, for example, to write:

$$k_{12} = P_1 Q_2 e^{-E_1 E_2 / rDkT}$$

where ε_1 and ε_2 are the actual charges on the radical 1 and monomer 2, D is the effective dielectric constant and r the distance of separation in the activated complex. The characteristic constant, e, would then be related to the actual charge, E, by the proportionality:

$$e_1 = E_1 / \sqrt{rDkT}$$

The relative rates at which monomers 1 and 2 compete for radicals of type 1 is thus given by an expression such as the following:

$$k_{11} / k_{12} = (Q_1 / Q_2)\, e^{-e_1(e_1 - e_2)}$$

$$k_{21} / k_{22} = (Q_1 / Q_2)\, e^{-e_2(e_1 - e_2)}$$

The constant P, connected with the reactivity of the radical, cancels out.

Consider, for example, the results reported by Lewis, Mayo and Hulse.[1] These authors investigated the copolymerization of all possible pairs among the four monomers: styrene, methyl methacrylate, acrylonitrile, vinylidene chloride. TABLE 2 gives their results for the relative reactivity of each monomer with each type of free radical.

TABLE 2

RELATIVE RATES OF MONOMER ADDITION

	Radical			
Monomer	Styrene	Acrylonitrile	Methylmethacrylate	Vinylidene chloride
Styrene	(1.0)	25.0	2.0	7.0
Acrylonitrile	2.5	(1.0)	0.8	2.7
Methyl methacrylate	2.0	7.0	(1.0)	4.1
Vinylidene chloride	0.5	1.1	0.4	(1.0)

These reactivities are consistent with the values for *Q* and *e* given in TABLE 3.

TABLE 3

Monomer	*Q*	*e*
Styrene	1.00	—1
Methyl methacrylate	0.64	0
Acrylonitrile	0.34	+1
Vinylidene chloride	0.16	0

This order of the four monomers fits in very well with the theoretical expectations based upon resonance stabilization as the principal factor influencing general monomer reactivity. Styrene, the most "reactive" of the set, exhibits the greatest number of resonance structures for the radical which is formed by addition to the chain. Methyl methacrylate comes second, acrylonitrile next, and vinylidene chloride last. In fact, no ordinary "conjugation" at all exists in the vinylidene chloride radical. A small amount of resonance stabilization might be expected from weakly contributing structures in which the carbon-chloride bond has a certain double-bond character analogous to that postulated in chlorobenzene.

The *"e"* values are consistent with the hypothesis that the double bond in styrene —and therefore the carbon atom with the odd electron in the styrene radical—is electron-rich (negative), the double bond in acrylonitrile is electron-poor (positive), while the double bonds in methyl methacrylate and vinylidene chloride are approximately "neutral". If this is true, one would expect the relative rate of addition of each neutral monomer to all radicals to be determined primarily by the overall reactivity of the monomer, and also for the competition of all monomers for the neutral radicals to be determined by the overall reactivities of the monomers. Furthermore, the rate of addition of styrene to styrene radical and acrylonitrile to acrylonitrile radical should be reduced by electrostatic repulsion to values smaller than those expected on the basis of the mean reactivities of the monomers; likewise, the rate of styrene adding to acrylonitrile, and *vice versa,* should be increased by electrostatic attraction beyond the values expected on the basis of mean monomer reactivities alone.

(5) It is now clear why the separate polymerization behavior of individual monomers is a poor guide to their copolymerization behavior. Styrene monomer is a *reactive* monomer, but the styrene-free radical is a stable, *unreactive radical.*

Vinyl acetate monomer is (relative to styrene) an *unreactive monomer,* but the vinyl acetate *free radical* is unstable and *reactive.*

These two effects tend to *compensate* one another. The self-growth of styrene (unreactive radical + reactive monomer) may be quite comparable in rate with the self-growth of vinyl acetate (reactive radical + unreactive monomer). But when vinyl acetate monomer and styrene monomer must compete for the *same* free radicals (as in copolymerization), the greater reactivity of the styrene is very evident.

REFERENCES

1. **Mayo, F. R., F. M. Lewis, & C. Walling.** 1948. J. Am. Chem. Soc. **70:** 1529.
2. **Alfrey, T., & C. C. Price.** 1948. J. Polymer Sci. **2:** 101.
3. **Walling, C., E. R. Briggs, K. B. Wolfstin, & F. R. Mayo.** 1948. J. Am. Chem. Soc. **70:** 1537.

CONFERENCES HELD

SECTION OF PSYCHOLOGY

CONFERENCE ON "HUMAN ENGINEERING": MAY 10 AND 11, 1948

The Section of Psychology held a Conference on "Human Engineering." Doctor Lawrence E. Abt, Research Division, College of Engineering, New York University, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Monday, May 10

Morning Session. Chairman, Jack W. Dunlap, Dunlap, Morris and Associates, Inc., New York, N. Y.

"Introductory Remarks," by the Conference Chairman.

"A Program of Human Engineering Research," by Leonard C. Mead, Special Devices Center, Office of Naval Research, Sands Point, L. I., N. Y.

"Handbooks of Human Engineering Data," by John L. Kennedy, Institute of Applied Experimental Psychology, Tufts College, Medford, Massachusetts.

Afternoon Session. Chairman, Leonard C. Mead, Office of Naval Research, Sands Point, L. I., N. Y.

"Problems Relating to Air Crews in Air Transport Design," by Ross A. McFarland, Research Division, Graduate School of Business Administration, Harvard University, Cambridge, Massachusetts.

"Some Considerations in Aero-Medical Research," by Lynn S. Beals, Jr., Special Devices Center, Office of Naval Research, Sands Point, L. I., N. Y.

"Human Efficiency as a Function of Light and Illumination," by Henry H. Hausner, College of Engineering, New York University, New York, N. Y.

"Theory and Methods for Analyzing Errors in Man-Machine Systems," by Alphonse Chapanis, Department of Psychology, The Johns Hopkins University, Baltimore, Maryland.

"Better Cockpit Lighting," by Clifford P. Seitz, Special Devices Center, Office of Naval Research, Sands Point, L. I., N. Y.

Tuesday, May 11

Morning Session. Chairman, Renato Contini, College of Engineering, New York University, New York, N. Y.

"The Basic Pattern of Human Locomotion," by Herbert Elftman, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"An Evaluation of Experimental Procedures Used in a Fundamental Study of Human Locomotion," by Howard D. Eberhardt and Verne T. Inman, University of California, Berkeley, California.

"Recent Developments in Lower Extremity Prostheses," by John Catranis, Catranis, Inc., Syracuse, N. Y.

"Biomechanical Studies of the Upper Limbs," by Craig L. Taylor and A. Blaschke, Department of Engineering, University of California, Los Angeles, California.

"Human Engineering Problems in Service Testing of Prosthetic Devices," by Lawrence E. Abt, Research Division, College of Engineering, New York University, New York, N. Y.

"A Preliminary Experiment on the Effect of Instrument Size and Graduation on the Speed and Accuracy of Scale Reading," by William E. Kappauf and William M. Smith, Department of Psychology, Princeton University, Princeton, N. J.

SECTION OF BIOLOGY

CONFERENCE ON "NEWER SYNTHETIC ANALGESICS": MAY 14 AND 15, 1948

The Section of Biology held a Conference on "Newer Synthetic Analgesics." Doctor M. L. Tainter, Sterling-Winthrop Research Institute, Rensselaer, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, May 14

Morning Session. "Greetings and Historical Introduction" by the Conference Chairman.

"Chemistry of Natural and Synthetic Analgesics," by Lyndon F. Small, National Institute of Health, Bethesda, Maryland.

"Experimental Methods for Studying Analgesia," by Carl C. Pfeiffer, University of Illinois, College of Medicine, Chicago, Illinois.

"A Critique of Analgesic Testing Methods," by Lloyd C. Miller, Sterling-Winthrop Research Institute, Rensselaer, N. Y.

Afternoon Session. Chairman, Raymond N. Bieter, University of Minnesota, Medical School, Minneapolis, Minnesota.

"Pharmacology of Metopon and Other New Analgesic Opium Derivatives," by Nathan B. Eddy, National Institute of Health, Bethesda, Maryland.

"Pharmacology of Demerol and its Analogues," by Frederick F. Yonkman, Ciba Pharmaceutical Products, Inc., Summit, N. J.

"Pharmacology of Methadon and Related Compounds," by K. K. Chen, The Lilly Research Laboratories, Indianapolis, Indiana.

Saturday, May 15

Morning Session. Chairman, Hans Molitor, Merck Institute for Therapeutic Research, Rahway, N. J.

"Animal Experimentation in Studying Addiction to the Newer Synthetic Analgesics," by M. H. Seevers, Department of Pharmacology, University of Michigan, Ann Arbor, Michigan.

"Methods and Results of Studying Experimental Human Addiction to the Newer Synthetic Analgesics," by Harris Isbell, U. S. Public Health Service, Federal Security Agency, Lexington, Kentucky.

"The Importance of Addiction to the New Synthetic Analgesic in Human Therapy," by Robert C. Batterman, New York University, College of Medicine, New York, N. Y.

"The Federal Food, Drug and Cosmetic Act and New Analgesic Drugs," by E. E. Nelson, Food and Drug Administration, Federal Security Agency, Washington, D. C.

"Regulatory Problems Under the Narcotic Law of the New Analgesics," by H. J. Anslinger, Commissioner of Narcotics, Bureau of Narcotics, Treasury Department, Washington, D. C.

Afternoon Session. Chairman, E. A. Rovenstine, New York University, College of Medicine, New York, N. Y.

"Use of the New Synthetic Analgesics in Internal Medicine," by Raymond N. Bieter, University of Minnesota, Minneapolis, Minnesota.

"Use of the New Synthetic Analgesics in Surgery," by E. A. Rovenstine.

"Use of the New Synthetic Analgesics in Obstetrics," by Bert B. Hershenson, Boston Lying-in Hospital, Boston, Massachusetts.

"Preliminary Experiences in the Use of Some of the Newer Analgesics in the Relief of Pain Due to Cancer," by R. L. Houde, L. H. Rasmussen, J. S. LaDue, and Frederick S. Phillips, The Sloan-Kettering Institute for Cancer Research, New York, N. Y.

CONFERENCE ON "ANTIBIOTICS DERIVED FROM *BACILLUS POLYMYXA*": MAY 21 AND 22, 1948

The Section of Biology held a Conference on "Antibiotics Derived from *Bacillus polymyxa*." Doctor Perrin H. Long, The Johns Hopkins University School of Medicine, Baltimore, Maryland, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, May 21

Afternoon Session. "Introductory Remarks" by the Conference Chairman.

"Historical Aspects," by P. G. Stansly, Stamford Research Laboratories, American Cyanamid Company, Stamford, Connecticut.

"Production and Isolation of Polymyxin," by J. N. Porter, G. Krupka, and

R. Broschard, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

"Effect of Various Factors on the Production of Polymyxin," by R. G. Benedict and F. H. Stodola, Fermentation Division, Northern Regional Research Laboratory, Peoria, Illinois.

"Aerosporin, Introduction," by G. Brownlee, The Wellcome Physiological Laboratories, Kent, England.

"Comparative Biological Studies of Polymyxin and Aerosporin," by H. J. White, C. Alverson, M. J. Baker, and E. R. Jackson, Stamford Research Laboratories, American Cyanamid Company, Stamford, Connecticut.

"Comparative Biological Studies of Aerosporin and Polymyxin," by G. Brownlee, S. R. M. Bushby, and I. E. Short, The Wellcome Physiological Research Laboratories, Kent, England.

Evening Session. "Chemical Comparison of Polymyxin and Aerosporin," by P. H. Bell, J. P. English, R. G. Shepherd, R. Winterbottom, J. F. Bone, C. E. Fellows, K. S. Howard, and M. M. Rogers, Stamford Research Laboratories, American Cyanamid Company, Stamford, Connecticut; and A. C. Dornbush, S. Kushner, and Y. Subba Row, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

"Comparative Chemical Studies of Aerosporin and Polymyxin," by J. R. Catch, T. S. G. Jones, and S. Wilkinson, The Wellcome Physiological Research Laboratories, Kent, England.

"Infrared Studies," by R. C. Gore, Stamford Research Laboratories, American Cyanamid Company, Stamford, Connecticut.

"Pharmacology of Polymyxin," by Morton F. Bryer, Emanuel S. Schoenbach, and Eleanor A. Bliss, The Johns Hopkins University School of Medicine, Baltimore, Maryland.

Saturday, May 22

Morning Session. "Pharmacology of Aerosporin," by G. Brownlee, S. R. M. Bushby, and I. E. Short, The Wellcome Research Laboratories, Kent, England.

"Polymyxin Assay Procedures," by E. T. Reese and G. M. Eisenberg, J. T. Baker Chemical Company, Phillipsburg, N. J.

"A Simple Assay Method for Polymyxin in Blood and Urine," by P. G. Stansly, Stamford Research Laboratories, American Cyanamid Company, Stamford, Connecticut.

"Experimental Treatment of *Brucella suis* in Guinea Pigs with Polymyxin," by C. Larson and B. N. Carle, National Institute of Health, Bethesda, Maryland.

"The Clinical Use of Polymyxin," by Emanuel S. Schoenbach, Morton S. Bryer, and Perrin H. Long, The Johns Hopkins University School of Medicine, Baltimore, Maryland.

"Remarks on Clinical Results with Aerosporin," by G. Brownlee, The Wellcome Physiological Research Laboratories, Kent, England.

CONFERENCE ON "THE MECHANISMS OF CELL DIVISION": MAY 28 AND 29, 1948

The Section of Biology held a Conference on "The Mechanisms of Cell Division." Doctor M. J. Kopac, New York University, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, May 28

Morning Session. Chairman, Robert Chambers, Marine Biological Laboratory, Woods Hole, Massachusetts, and New York University, New York, N. Y.

"Cleavage in Marine Eggs," by Edwin G. Conklin, Princeton University, Princeton, N. J.

"Cell Division with Special Reference to Cells in Tissue Culture," by Warren

H. Lewis, Wistar Institute of Anatomy and Biology, Philadelphia, Pennsylvania.

"Cytoplasmic Changes During Cell Division with Reference to Mitochondria and the Golgi Substance," by A. J. Dalton, National Cancer Institute, Bethesda, Maryland.

"Experimental Control of Fission in *Amoeba proteus*," by H. W. Chalkley, National Cancer Institute, Bethesda, Maryland.

Afternoon Session. Chairman, E. Newton Harvey, Princeton University Princeton, N. J.

"Structural and Kinetic Aspects of Cell Division as Revealed by Micrurgical Methods," by Robert Chambers, Laboratory of Experimental Cell Research, Marine Biological Laboratory, Woods Hole, Massachusetts.

"The Action of Hydrostatic Pressure on Cell Division," by Douglas Marsland, New York University, New York, N. Y.

"Cleavage in Centrifuged Eggs and in Parthenogenetic Merogones," by Ethel Browne Harvey, Princeton University, Princeton, N. J.

"The Effects of Ultracentrifugal Force on the Cell, with Special Reference to Cell Division," by H. W. Beams, State University of Iowa, Iowa City, Iowa.

Saturday, May 29

Morning Session. Chairman, Austin M. Brues, Argonne National Laboratory, Chicago, Illinois.

"The Action of Colchicine on Cell Division in Human Cancer, Animal and Plant Tissues," by Michael Levine, Montefiore Hospital, New York, N. Y.

"The Inhibition of Cell Division by Substituted Phenols, with Special Reference to the Metabolism of Dividing Cells," by G. H. A. Clowes, Lilly Research Laboratories, Indianapolis, Indiana.

"The Action of Nitrogen Mustards and Related Substances on Cell Division," by Jonas S. Friedenwald, Wilmer Ophthalmological Institute, Johns Hopkins Hospital, Baltimore, Maryland.

"The Action of Podophyllin and its Fractions on Marine Eggs," by Ivor Cornman, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"The Effects of Some Mitotic Inhibitors on Tumor Cells," by Ross C. MacCardle, National Cancer Institute, Bethesda, Maryland.

Afternoon Session. Chairman, G. Failla, Columbia University, New York, N. Y.

"The Effects of External and Internal Radiation on Cell Division," by Austin M. Brues, Argonne National Laboratory, Chicago, Illinois.

"The Effects of Ultraviolet Radiation on Cell Division," by Alexander Hollaender, Oak Ridge National Laboratory, Oak Ridge, Tennessee.

"Radiation Sensitivity of Cells During Mitotic and Meiotic Cycles, with Emphasis on Possible Cytochemical Changes," by Arnold H. Sparrow, Brookhaven National Laboratory, Associated Universities, Inc., Upton, Long Island, N. Y.

"The Probable Significance of Ultrastructures in Cell Division—a Résumé," by M. J. Kopac, New York University, New York, N. Y.